THE LANCET.

VOL. II.] LONDON, SATURDAY, SEPTEMBER 13, 1834. [1833-34.

LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XLVII.

ON THE SANGUIFEROUS SYSTEM IN THE RADIATED AND ARTICULATED CLASSES.

THE interior of living animals presents everywhere a busy scene of movement and activity. Myriads of vital currents meander incessantly through every the minutest part of the solid bones, and through the almost inert and vegetative ligaments. The nervous system is in constant activity, conveying to its centre, or radiating to its periphery, the mysterious influences which affect it, and the organs of the senses enjoy no repose from the ceaseless motions and impulses of surrounding nature. The muscular system is formed solely for motion, and is never at rest. The intestines and their heterogeneous contents are moved by this system incessantly, like the waves of the sea, and every gland of the body is a busy laboratory, where complicated chemical processes are constantly going on throughout life. Half of the animal structure is a tissue of tubes of almost invisible minuteness, in which fluids are in continual motion, some leading into the blood fresh supplies received from without, others conveying nutriment to every point of the system, and others removing the decayed materials from the body. There is a continuous circle of renovation and decay which must contain within itself the principle of its beginning and its end. The organs of animal life feed the vegetative, but it is only through the vegetative that the animal organs enjoy their power and their existence. We have seen that all the organs of relation, even the most elevated, the nervous system itself, and all the complicated organs of the senses, serve but to feed the stomach; the stomach feeds the lacteals, the lacteals the bloodvessels, and the bloodvessels convey nutriment and life to every point of the system, and to the organs of relation which select and procure their materials. Indeed these vortex-like movements constitute the only manifestations of life; and death is only that condition in which they are incapable of being continued, so that the life is in the blood as much as in the brain or the heart.

The bloodvessels being a mere extension of the alimentary canal to every part of the body, we find the sanguiferous system, under various forms, nearly as universal in the animal kingdom as the digestive itself, and they keep pace with each other in the march of their development. This system, like most others, is developed from the periphery to the centre, and, like the nervous system, commencing with globules and filaments, this complex hydraulic apparatus commences with moving fluids and capillary tubes, which develop vascular trunks and at length a central pulsating dorsal vessel. This lengthened vessel becomes concentrated to form a muscular ventricle, which develops an auricle by its division, and these two primary cavities, by each again successively dividing, produce first the trilocular heart of the amphibia and reptiles, and then the quadrilocular heart of the warm-blooded vertebrata. We observe a regular gradation in the complexness of this system, from the lowest to the highest classes in its adult or permanent forms, and those forms are repeated with remarkable exactness and regularity in the successive conditions through which this system passes during its development in man and in all the higher vertebrated animals.

In the radiated classes of animals, as in the earliest embryo-condition of the human body, vessels are found only to contain and circulate the fluids, without an auricle or a ventricle; and, indeed, in some of those animals the fluids appear to meander through open passages in the cellular tissue of the body, without being confined to distinct vessels, in the same manner as we see the globules moving in the germinal part of the ovum during the first efforts at development, and before vessels are formed in the vertebrated animals. In the larger polygastric animalcules, and probably in all, we can perceive, by using highly magnifying powers, a beautiful reticulate appearance of minute transparent colourless anastomosing vessels spread over the surface of the body. This capillary circulation has yet no centre developed; no dorsal vessel or pulsating ventricle yet directs or impels the currents; and the minute vessels themselves, in which the transparent fluids circulate over the surface of the animalcules, appear to be as motionless as the cells of a plant, while their contained fluid revolves in its smooth and constant gyrations. This movement of fluids in the reticulate and apparently inert tubes of the polygastric animalcules, resembles the currents of vital fluids, replete with globules, pointed out by Schultz in the vascular plexuses of the stipulæ and other parts of plants; but we do not observe in the circulating system of the animalcules, even that repulsive nucleus of the currents, like a rudimentary heart, which is seen in the cells of many plants.

No circulation of a distinct vital fluid has been observed in the soft gelatinous substance of the poriferous animals, but all parts of their interior are aerated and nourished by the currents of water which pass incessantly through their meandering, ramifying, anastomosing canals. The circulation of the blood, however, in a great variety of zoophytes was minutely described fifty years ago by Cavolini, especially in *sertulariæ*, *plumulariæ*, *campanulariæ*, *tubulariæ*, and other soft flexible transparent genera, and numerous succeeding observers have pointed out the same phenomenon in other zoophytes, as Esper, Olivi, Ehrenberg, Chiaje, Fleming. Cavolini observed in a longitudinal cavity of the fleshy centre of most of these zoophytes a fluid in motion, containing distinct globules like those composing the flesh, which continued during the whole life of the animals to ascend and descend through all the stems, and the branches, and the polypi. These globules, he observed, were contained in a thin, transparent, colourless fluid, not at first very obvious, and he saw them carried upwards and downwards through the fleshy axis, and sometimes transversely in the cavity. He compared the circulation of zoophytes to that of the larvæ of insects, and he generally terms the fleshy central cavity of the stem of all these zoophytes in which the digested fluids are seen circulating, the heart of those animals. These motions of a circulating fluid containing numerous globular particles, he observed in the interior of the fleshy axis in all those tubular keratophytes, which were sufficiently transparent to allow their interior to be perceived through the microscope. Cavolini describes at great length the nature and direction of the currents which he observed in the axis of the *campanularia dichotoma*, and in *plumulariæ*, and different species of *sertulariæ*. And indeed the same motions in the fleshy axis were observed by Lœffling, and described by Pallas in the *sertularia geniculata*, before the time of Cavolini. But Olivi, who more than forty years ago entered into a detailed examination of the circulation of zoophytes, especially of the irregular longitudinal vortex-like currents seen in the fleshy axis of *sertulariæ*, conceived them to depend on the food and water swallowed by the polypi, which were subjected thus to a peristaltic motion to assist in their digestion. Delle Chiaje considers the ciliated tentacula of zoophytes as respiratory organs, and that the circulating fluid of the axis is sent up to those organs to be aerated. Dr. Fleming observed a similar circulation of globules in the cellular substance of the stem of the *campanularia gelatinosa*; and similar vortex-like motions which I have described in the polypi of *flustræ*, *virulariæ*, *pennatulæ*, and other zoophytes, I have been inclined to refer to the action of minute vibratile cilia—the common agents of all analogous movements in the lowest tribes of animals, and which have often been seen in the interior of the intestine, and on its exterior in higher animals. Sanguiferous vessels have been observed by Delle Chiaje extending from the base of the polypi in the *coral* and *gorgonia*, and forming a superficial reticulate plexus in the *cariophylia*. Nordmann has described the circulation of the blood in the *alcyonella diaphana*, which he compares to that of the *chara*.

In many of the acalepha, ramified canals are seen extending from the stomach through the cellular substance of the mantle by which the digested part of the food is conveyed to distant parts of the body, but an isolated sanguiferous system has not been observed in the soft transparent body of those animals. Water is admitted

in large quantity, and by various means into the interior of the body in most of the marine invertebrata to aerate more extensively their contained fluids. Different passages for the transmission of this element are seen in the acalepha, and in the *beroe pileus* it moves like a circulating blood through wide longitudinal lateral tubes, the parietes of which appear through the transparent sides of the animal to remain perfectly motionless, while the fluid circulates in their interior, like the sap in a vegetable cell.

In the class echinoderma, we observe a distinct mesentery developed in the asterias, in the holothuria, in the echinus, in the spatangus, and many others of those radiated animals; and we perceive a distinct vascular system, consisting chiefly of minute capillaries ramified upon the membrane which connects the intestine throughout its whole course with the exterior parietes of the animal's body. This presents us with an example of a sanguiferous system, consisting merely of vessels, without either an auricle or ventricle. This circulating system is obvious in the *asterias*, where we observe it in the radiating divisions of the body, originating by numerous small capillaries near to that upper part of the body to which the cæca extending from the stomach are connected, and directing their course, enlarging by the anastomosing of those capillaries, into larger and larger trunks, and proceeding towards the centre or disk of the body. They appear to form a circular trunk, which surrounds the stomach, and in the course of these numerous bloodvessels no general pulsating muscular reservoir or heart appears to exist, although one of the vessels has received that name. Thus it still appears, that in the simple conditions of the vascular system met with in the radiated animals, it consists merely of bloodvessels, without either auricle or ventricle being developed or required. Some of these vessels of the asterias are seen to spread on the viscera—on the stomach, the cæcal appendices, and the ovary, and their contained fluid is aerated by the water taken into the general cavity of the body. A great intestinal artery passes from the circular trunk around the stomach, into each division of the body, and divides into two branches, to be distributed on the two ramified cæca in each. There are five trunks distributed on the vesicular feet of the five rays, and five other trunks continue along the whole rays to be ramified on the segments and superficial parts of the body. In some species of asterias there is a cordiform enlargement, considered as a rudimentary ventricle, which is wanting in other species. In the *echinus* there is also a circular vessel around the mouth, which sends out branches to the alimentary canal, and five branches to form a vascular ring around the anal aperture. A large vein passing forwards along the inner part of the mesentery enters this arterial ring around the mouth, and the arteries for the tubular feet come off from this oral ring, as in the *asterias*. In the long vermiform *holothuriæ* the sanguiferous system still consists merely of arteries and veins which have their trunks chiefly distributed on the lengthened and convoluted intestine, and on the ramified respiratory apparatus opening into the cloaca, as you observe beautifully represented in these large folio plates of TIEDEMANN. You perceive a large arterial trunk running along the exterior margin of the intestine, ramifying on that organ, diminishing to a small vessel at both ends, and sending a small arterial ring around the œsophagus. The blood thus distributed on the alimentary canal, is collected on the inner part of the convolutions by innumerable capillary veins, which have probably received the chyle in their course over the intestine. These anastomosing intestinal veins form large venous trunks, which are chiefly distributed on the two great and complicated respiratory organs placed between the turns of the intestinal canal. The circular, œsophageal artery around the anterior extremity of the intestine gives off five branches, which pass forwards to supply the apparatus of the mouth, and the general integuments of the body. These five longitudinal arteries extending backwards parallel to the muscular bands, and between the muscles and the skin, supply also the tubular feet of these animals. In the *sipunculus*, however, the sanguiferous system approaches, like the general form and structure of the body, more nearly to the regular and symmetrical character presented by the helminthoid articulated classes. There is a median longitudinal arterial vessel in this animal, giving off numerous lateral branches, and which appears to be enlarged at both extremities, forming what have been considered as a rudimentary auricle and ventricle. But although DELLE CHIAJE has given these names to parts of the sanguiferous system of this animal, and CUVIER has given the same names to parts of the vascular system of the annelides, we must rise much higher in the scale before we find these two first cavities of the heart distinctly developed

The long cylindrical form of the trunk in the articulated classes of animals we have seen to impress that form on most of the internal organs, and we find it espe-

cially marked in the longitudinal direction and lengthened form of the great centres of the vascular system. The blood of the entomoid classes, which is often of a deep-red colour, is seen to be extensively distributed by large trunks through the body, and it is chiefly in the form of the great muscular pulsating centre that we perceive the higher development of this system of the entomoid classes.

In many of the intestinal worms we observe on each of the two sides of the body a longitudinal filament, situate between the bands of the inner muscular coat, as in the ascaris, the echinorhynchus, and many others of the same long cylindrical form. Those appear to be two blood-vessels in which there is contained a colourless or reddish fluid that is received from the appendices of the intestine, and is probably distributed partially through the body by minute capillaries. The dorsal filament is probably an artery. Now the circulation has been carefully traced in a great variety of entozoa, especially by the laborious and skilful naturalists of Germany, and there is considerable difference in the mode of distribution of the vessels, according to the general form and the extent of development of the animals. Distinct arteries and veins are seen extensively ramified through the body, and possessing distinct membranous parietes, but the motions of the thin colourless blood are not always perceptible in these vessels, and where the blood is seen in motion through the vessels, as in the *diplozoon*, the *aspidogaster*, and the *diplostomum*, no motion is seen in the vessels themselves to impel the blood. In some, as the *caryophyllæus*, vessels are seen extending as usual through the body, but no fluid can be detected, in the living state, by the aid of highly magnifying powers passing through these vessels, probably from the want of globules to render the currents perceptible. In some of the higher entomoid forms of this class, as the *echtheres*, a distinct pulsating lengthened cavity is seen in the middle of the cephalothorax, which sends vessels laterally to the arms and posteriorly to the viscera, thus approaching to the form of the sanguiferous system of the crustacea; but the blood in these vessels, when examined in the living state, appears to pass backwards and forwards, to and fro, in the same passages, as if they performed the functions both of arteries and veins. Ehrenberg observed a distinct motion of the blood through the vessels of the *distomum militare*.

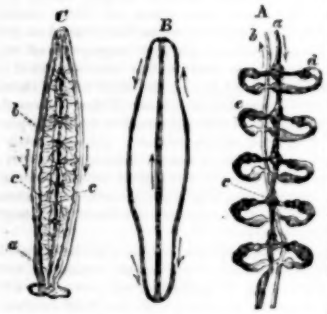
From the great transparency and colourless texture of the rotiferous animals, extremely minute parts can be detected in their interior. That peculiar vibratory surface produced by the action of minute cilia which we sometimes see on the exterior surface and in the interior of the alimentary canal in aquatic animals under the microscope, and which appears to have been mistaken by Corti and Gruithuysen for a circulation of blood, is seen on the intestine, and also in its interior in some of the wheel-animalcules. Ehrenberg has observed these cilia vibrating on the exterior surface of the intestine in the wide abdominal cavity of the *hydatina senta*. There is, however, a distinct sanguiferous system in these animals, which is formed upon the plan of that of the articulated classes. In the *hydatina senta* there is a longitudinal dorsal straight vessel, which extends nearly along the whole body, and gives off nine lateral pairs of branches to be distributed on the nine partially marked segments of the body, but no motion has been detected in those vessels nor in the fluid within them. The sanguiferous system of the cirrhopoda seems also to be constructed upon the plan of that of the articulated animals, with a dorsal pulsating vessel, discovered by Poli, which receives the blood from the branchiæ, as you observe represented in these plates of Cuvier, and distributes it through the system.

The sanguiferous system of the annelida presents still a very simple condition with regard to its great central parts, but with regard to the colour of the fluid contents of those vessels, this is one of the most anomalous classes of the invertebrata. They contain not that transparent bluish-white or almost colourless fluid which we observe in most of the other invertebrata, but, on the contrary, they convey through their arteries and veins a red-coloured thin fluid with a considerable degree of transparency. It is not the thick opaque fluid which abounds so much in fibrine and globules which we find circulating in the vessels of the vertebrated classes. We can scarcely perceive the fibrine in those red-blooded worms. The globules of their blood are still few.

We generally observe in the class of red-blooded worms two inferior lateral vessels which direct their course backwards, lying towards the abdominal surface of the body. Those inferior lateral vessels are two venous trunks that receive the blood from all parts of the body as they pass along towards the posterior part. Those venous trunks generally terminate, as you observe in the large diagram of the circulation in the *erpobdella vulgaris* (*Fig.* 168, *C*) before you, at the posterior part of a great longitudinal vessel. That longitudinal vessel (*Fig.* 168, *a b*) is generally dorsal, and presents distinct pul-

sations, which are easily seen through the transparent body of many of these delicate worms, such as this *pectinaria*, and presents a beautiful spectacle of the deep-red currents meandering through the soft, transparent, and colourless texture of the body. We observe the dorsal vessel of the living *pectinaria*, which is very common on our sandy coasts, filling with red blood, poured into it from inferior vessels behind, and by its peristaltic contractile motion forcing that red-coloured fluid forwards, and distributing it through the system. The two returning lateral veins (*Fig.* 168, *c c*) are seen in this *er-*

Fig. 168



pobdella, conveying the blood backwards. There are considerable modifications introduced into this general plan of the vascular system of the red-blooded animals, in consequence of the development of their respiratory organs, and in consequence of the forms and positions being various which those respiratory organs present, they being sometimes situate on the head, sometimes along the back, and sometimes they are in the form of internal cavities opening by pores from the sides of the body. This diagram (*Fig.* 168, *B*) of the circulation of the erpobdella represents the most frequent course of the blood through the body, not only of the annelides, but of all the other articulated classes. You observe the dorsal arterial trunk passing up the middle, and the two abdominal veins returning backwards along the lateral parts of the body. Duges observed in the *prostoma*, belonging to the *planariæ*, the same direction of the blood forwards in the middle longitudinal artery, and backwards in the two lateral veins, as you see represented in these figures of his second memoir on the *planariæ*, and he perceived also two little vesicular enlargements upon these veins, analogous to the more numerous vesicles or sinuses which are seen on the vessels of the earth-worm, and many other annelides. He has also observed these vesicles on the vessels of several species of *planaria*, and they are seen on those of the *nais*. From the free communication which he found between these lateral vessels and the ovaries of several planariæ, he conceives that in these animals the sanguiferous vessels may perform the function also of oviducts, but his observations on the systole and diastole of these vessels are at variance with those of Ehrenberg, or the vessels of the *planaria lactea*. In the *planaria viganensis* there are two vesicular enlargements on the anterior portion of each of the lateral vessels, and, posteriorly, these vessels send each a large trunk to the ovary and cloaca, as represented in the figures of Duges. In the nais, Gruithuysen observed the blood conveyed forwards in the usual middle dorsal artery, which at the anterior part of the body forms an arch round the neck, descending on each side of the œsophagus to form the inferior single median vein which conveys the blood backwards. This anterior descending vascular ring of the nais exhibits the same strong motions of systole and diastole, which you see in the corresponding dilated and sacculated rings (*Fig.* 168, *A*, *d e*) passing down from the anterior part of the great dorsal vessel of the earth-worm (*Fig.* 169, *b*), as here carefully delineated in the plates of Morren. The number of sacs upon these anterior arches of the earth-worm is not always the same as represented in these plates of Morren, where you observe the abdominal vein (*Fig.* 168, *a e*) also sacculated; they vary both in number and form in the same species and in the different arches of the same animal. I have sometimes found six of these sacs on one side of a single arch. They are like the branchial arches of the aorta of vertebrata. By keeping the earth-worm for a short time from food, and until it discharges the black earth which usually fills its intestinal canal, you can thus easily perceive the waves of red blood passing forwards in the dorsal vessel, and propelled forcibly downwards to the abdominal surface by these five sacculated lateral arches (*Fig.* 168), which Morren has considered as so many auricles and ventricles of these worms. This, however, is like the analogy of Baron Cuvier, who considered the dorsal vessel of the annelides as a ventricle, and the two long abdominal or lateral veins as two auricles, and thereby ranked these worms above all the other articulated classes,

above the insects and crustacea, which tends to obscure the order and harmony of their development, and to conceal their natural affinities. Some have imagined that the blood in the dorsal vessel of the earth-worm moves backwards, which is a mistake, arising, I think, from the manner in which the vessel sinks beneath the hepatic follicles during its contraction. The pulmonary arteries of the earth-worm appear to arise from the sides of the inferior median vessel, which is generally in close connexion with the ganglionic columns in the annelides, and they give off minute branches in their course upwards and laterally to the air-sacs. These minute branches from the sides of the great pulmonary trunks of each segment, are represented by Delle Chiaje as terminating in minute vesicular enlargements, which are common on the vascular system of the annelides, as they are on the respiratory vessels of insects. You observe many of these round vesicles represented on the bloodvessels of the common leech, in the coloured plates of Delle Chiaje and Muller, and they were observed by Muller to receive and discharge the red blood. Muller found the blood to ascend by one lateral trunk, and to descend by the trunk of the opposite side in the common leech. We generally perceive on the small median longitudinal abdominal trunk which accompanies and is closely connected with the nervous columns of the annelides, numerous small vesicular enlargements in its course, which correspond with the position and number of the attached nervous ganglia, as you see in these plates of Muller of the sanguiferous system of the leech. So that the sanguiferous system of these red-blooded worms is somewhat in the condition of development which we find in the lymphatic system of the vertebrata, with its numerous pulsating heart-like cellular vesicles.

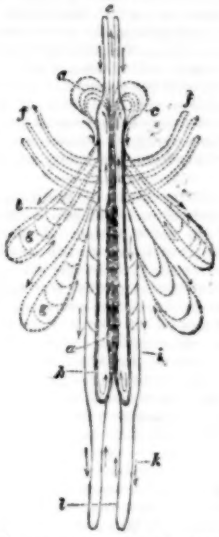
The uniformity of the plan of the sanguiferous system is more obvious and more constant in the entomoid classes than it appears, from our present knowledge, to be in the helminthoid forms of articulata. It appears to be in the higher forms of these entomoid classes that we first find a capacious and strong muscular sac developed on the great arterial trunk, which receives the whole of the blood and propels it throughout the system. The nearest approach to the vascular system of the annelides is seen in the myriapods, those pedigerous worms with hardened integuments, and they are justly regarded by Strauss as presenting nearly the simplest form of this system. The great dorsal vessel is here still a narrow tube extending along the whole trunk of the body. In the scolopendra this dorsal vessel begins from the last or caudal segment, and is continued forwards to the segment behind the head, where it becomes smaller, and is continued in the same median line to near the mouth, where it becomes imperceptible. At the commencement of this narrow anterior portion or aorta, two lateral trunks are given off, as we see also given off from the more concentrated heart of the higher crustacea. A second pair of vessels is seen to originate from the arterial trunk in the head, and a third pair is given off near to the mouth, which supplies the neighbouring organs. This long dorsal vessel is retained in its situation by the same muscular lateral bands, which we see suspending this organ in the other entomoid classes. This condition of the vascular system of the myriapods is analogous also to that which we find in the larva state of the higher insects, when the dorsal vessel is in its simplest and least concentrated form, with its valvular apparatus the least developed.

In the class of insects the vascular system has long been a subject of much and of interesting inquiry, both on account of the changes it presents during the growth and metamorphoses of the insects, and also on account of the various forms and conditions of development which that vascular system presents in the different orders of insects. It was at one period believed that the dorsal vessel of insects was a sac shut at both extremities, and of a glandular nature, and that it was destined by its peristaltic action upon a continued honey-like substance to form the fatty substance of the bodies of insects. It was imagined that the circulating system was the less necessary in insects on account of the extensive distribution of the respiratory organs throughout all points of the body. By examining insects, however, newly escaped from their larva covering, and by examining them with great care in the larva state,—those which have the surface of the body sufficiently transparent and of a size so small as to be conveniently subjected to high magnifying powers, — it was seen by many accurate observers, as Lieuwenhoek, Nitzch, and Baker, that there is a distinct motion of globules in different parts of the body. More especially it was observed by Baker that a motion of globules was obvious, extending forward along the middle of the back, and extending backwards along the sides of the caterpillar, and thus completing the circulation. These currents, directed backwards, are situate, as in the annelides, upon a lower plane in the trunk than the dorsal vessel itself. For a considerable

time, however, on account of the opacity of the parts of the head, no opening or ramification was perceived at the anterior extremity of the dorsal vessel so as to allow of the distribution of its contents through the body; therefore, various conjectures were had recourse to regarding the course the fluids took in moving through the body. Some imagined that by the contraction of the dorsal vessel the fluid contained within it was propelled through an orifice at its anterior extremity, was thrown thus into the cavity of the abdomen, and that it gained admission again into the posterior part of the dorsal vessel, and thus performed a kind of circular motion. By more careful and repeated examination, however, of minute transparent larvæ, CARUS succeeded in pointing out the course of the blood in various parts of the body, and at length in showing the subdivision and ramification of the anterior part of the dorsal vessel in the head. The fluid which circulates in those passages and vessels of insects is transparent and colourless, consisting of globules, which, by a transmitted light, throw up a slightly brownish colour to the eye; and those are contained in a more homogeneous attenuated thin serous fluid, colourless also. The globules in these animals appear comparatively large, on account of their being more isolated on account of the smallness of the vessels, the smallness of the animals, the smallness of all the ordinary objects of comparison around them while they are moving under the microscope. This thin serous part of the blood seems to constitute the whole mass of the circulating fluid in the lowest classes, and the proportion of the fibrinous or globular part increases as we ascend through the classes of animals.

The structure of this dorsal vessel, of insects (*Fig.* 169, *a b*), which conveys the blood from behind forwards, has of late years been made the subject of careful examination by STRAUSS, who has given these numerous magnified views of its entire form and internal structure. In the larva state of insects, as in the simpler annelides, the dorsal vessel has a more lengthened, narrow, thin, and uniform appearance, than in insects which have passed on to the chrysalis condition, or their imago state. We observe, indeed, on watching the same insect in passing through its metamorphoses, that this dorsal vessel becomes thicker in its parietes, contracts in the cavity of the abdomen, and by the closer approximation of the segments themselves, the contraction of the muscles, as we saw before, gradually pulling the segments within each other, all the contained parts, as well as this vessel, are shortened. The two nervous filaments which connect the ganglia, and extend along the abdomen, are by this rendered zigzag, and the ganglia are brought nearer to each other. The same effect is

Fig. 169



observed upon the dorsal vessel. We thus perceive the formation and increase of the valves, which valves are bilabiate folds of the coats of the vessel itself. These valves, according to STRAUSS, are formed by a reflection inwards of the dense, tough, exterior general covering of this artery. The dorsal vessel appears like a series of eight chambers (*Fig.* 169, *a b*), pushed within each other, the posterior always within the one before it, so that the loose valvular folds are naturally directed forwards. When we examine the interior of this dorsal vessel, in its perfect state, we see that the valves produced by those folds passing inwards are confined to the abdominal portion of this longitudinal vessel, and form there a pulsating sac for each segment. Those folds pass inwards in such a manner, that the moving fluid is admitted by them to pass only forwards; but as those partially separated cavities of the dorsal vessel, eight in number, thus produced by the constrictions formed by the internal valves, contract in succession from behind forwards, the fluid is propelled forwards from one chamber to another, and cannot recede. It cannot

recede, on account of the *vis a tergo*—on account of the rush of globules and serum pouring in from the two abdominal vessels that enter the dorsal vessel from behind, pushing the current of blood forward. When the blood has got into the first or posterior cavity (*Fig.* 169, *a*), its contents, by the contraction of that sac, are propelled forwards into the second, this anterior cavity being partially emptied by the previous contraction in expelling its contents. Each of those eight abdominal portions of the dorsal vessel of insects, therefore, performs the function of a heart belonging to each segment, and they each receive two lateral currents of blood poured into their posterior ends from the cavity of the abdomen.

Now this succession of pulsating cavities is interesting, when you consider it with relation to the systems of those animals which we have already examined; for we have seen that the ganglia are the same within each segment; that there are nerves of motion and of sensation within each segment; that the muscular apparatus of each segment is of the same form as that of the succeeding segment; and that even the portions of the digestive organs, or the alimentary canal in the simplest condition, are also nearly the same throughout the several segments of the body, as we see here in the vascular system also. We perceive thus that each segment is nearly a complete organization, like the segments of a *tænia*, and that in each segment we have got almost a complete vascular system. All the parts of the body in insects, however, are much more closely connected together, and less capable of independent existence, than in the inferior helminthoid classes. Now those eight cavities are observed to receive currents of blood from the abdominal cavity by each of their sides. When we magnify very much, in the young transparent larva, the dorsal vessel, under a favourable light, we can see a rushing of globules towards the posterior part of each of these cordiform segments of the dorsal vessel. We can see that there is a distinct admission of blood-globules into each of those parts of the dorsal vessel, as into a separate heart, and that their pulsations continue in succession from behind forwards, the posterior pulsating first, and the anterior last. This introduction of lateral streams of globules into the dorsal vessel was ascribed by my ingenious friend Strauss to the passage of the blood from an exterior enveloping auricle into the cavity of the dorsal ventricle or vessel, and he considers this exterior enveloping auricle as common to the entomoid classes. From this dorsal heart or ventricle the blood passes forwards into a vessel (*Fig.* 169, *b c*) of thinner coats, which we can very distinctly perceive as it advances through the thorax to the head. This vessel, continued from the heart, is the aorta, and is much more uniform in its diameter, and appears, for a considerable distance, to give off no trunks. At the anterior part, however, of this lengthened cylindrical aorta, continued from the heart, it comes to the opaque part of the head, which so long rendered its further course a subject of difficult investigation with anatomists. There it has been found that the anterior part of this aortal vessel divides into numerous arches (*Fig.* 169, *c d*), which arches radiate chiefly upwards, laterally, and backwards, giving off currents to the antennæ (*Fig.* 169, *c e*) and other parts of the head. There we lose sight of the appearance of distinct membranous coats and distinct membranous vessels for containing the circulating blood. Those arches do not appear to subdivide, as in red-blooded worms, into minutely ramified capillaries. There is scarcely indeed such a delicately ramified structure seen in the vascular system of the class of insects as we see even among the lowest of the annelides. The streams of globules are seen to meander through different parts of the cellular tissue of the body, and to form loops, curves, and circles, passing out and returning, but not ramifying and extending into minute capillaries, like the accompanying air-vessels. The arches (*Fig.* 169, *d*) of the head extending laterally and backwards, give off loops or currents to the antennæ and other parts of the head, and at length converge and terminate in the two great inferior or abdominal trunks (*Fig.* 169, *h*), or currents, situate towards the anterior or ventral surface of the insect, and which convey the blood backwards. In their course backwards they give off not, apparently, membranous tubes or vessels, but they allow currents of globules directed to various organs to escape from the main streams, which diverging currents again return to the lateral streams directed backwards. Thus as the streams arrive opposite to the branches of the legs, a single current is seen to pass into each of the legs (*Fig.* 169, *f f*). The globules are seen to run in that direction for a short distance into the leg, and sometimes through its whole length, and then they appear again to turn and come back into the main two lateral currents (*Fig.* 169, *h*) that pass along the inferior part of the abdomen backwards to the posterior end of the dorsal vessel. This is observed in the diagram (*Fig.* 169, *f f*) passing to the extremities of the six legs of the insect. The distribution of the blood in the wings (*Fig.* 169, *g g*) of in-

sects is much more extensive, and is better known. No parts are more diversified in structure than the wings of insects, and the forms which they present are more various. So it is with regard to the distribution of the blood through those wings. Many figures have been published of the mode in which the blood circulates through the wings of different species of insects, taking the insects at the period at which they first escape from their larva or their chrysalis condition, when the wings are yet colourless, soft, and transparent, and when the currents are much more numerous through them than in the adult state. It is necessary to take the wings in that soft and transparent condition, both because parts of the vascular system of the wings, as of other external parts of the body, at an adult period, distinctly become obliterated, and because of the increased hardness, dryness, and opacity, which the future development of the wings, and the action of the air upon them, produced, rendering it nearly impossible to make microscopical observations on the movements of those streams of globules which may still partially extend over the wings. In differently formed wings the blood is observed to take different courses, but you will generally observe that along the upper part or anterior thick margin of the expanded wing (*Fig.* 169, *g g*), the blood is directed from the lateral stream of the abdomen outwards. Those currents of globules describe different curves over the surface of the organ, anastomose freely, and, at length, in the posterior part or margin of the wing the currents of globules are seen to pass again towards the main abdominal currents (*Fig.* 169, *h*), which convey them backwards to the end of the dorsal vessel, into which they enter along with all the currents from the abdominal viscera, the caudal appendices, and the other parts which have been observed to receive distinct streams of this extensively circulated fluid in the body of insects.

I have placed before you in this large coloured diagram (*Fig.* 169), the principal discoveries which have been authenticated regarding the distribution of the circulating system of insects. There is here a great variety of parts belonging to different insects, placed together upon one diagram, so that it presents to you a view of the recent observations of Professor Carus on the distribution of the great aortal vessel in the head (*Fig.* 169, *c d*); of the recent observations of Strauss on the structure and form of the dorsal vessel (*Fig.* 169, *a b*) and its valves. The same diagram presents to you four wings (*Fig.* 169, *g g*), exhibiting four different though ordinary modes of the distribution of blood in those organs. Numerous currents, and apparently contained in this vessel, are seen moving to and from the principal viscera of the trunk. And exterior to the two great lateral abdominal streams (*Fig.* 169, *h*) of blood-globules running backwards along the inferior portion of the trunk, two exterior smaller currents (*Fig.* 169, *i*) of globules are seen, more towards the outer or lateral margins of the insects, nearer to the exterior openings of the respiratory organs. These are small accessary currents which receive the blood from all the exterior parts in the immediate vicinity, and convey blood, at different distances, into the two principal abdominal currents that are directed backwards. Vessels have been seen in several insects, passing from the dorsal vessel to different organs of the trunk, especially to the oviducts, by Muller, and to the biliary organs. We observe that similar currents of globules, in some of the insects that have long caudal appendices (*Fig.* 169, *k l*), continue downwards in the form of loops detached from the abdominal currents, through those codal appendices; but these caudal currents (*Fig.* 169, *k l*), like those of other appendices of the trunk, are not ramified and spread out in minute capillary streams, as we see in animals which have the vascular system more developed and more important than in those little dry-bodied aeronauts, organized to flit, like shadows, but for a moment through the attenuated air.

The sanguiferous system is more highly developed in the arachnida than in insects, especially in the form and muscular structure of the heart. This organ is almost always fixed, as in insects, by means of transverse muscular bands to the dorsal part of the segments, but these appear to be wanting in the phalangium, where the heart extends quite free along the middle of the back. The heart of the scorpion is a lengthened, wide, and muscular canal, tapering and narrow at both ends, fixed by muscular bands, and giving off numerous lateral branches in its course, especially to the hepatic organs developed from the sides of the intestine. It presents five enlargements of its caliber, and from these enlarged dilated portions which are the parts fixed by the transverse muscular bands, the vessels are given off to the neighbouring organs. This organ is shorter proportionally in the spiders, and is very wide at its anterior portion, where it suddenly tapers; but it is narrow and contracted at its posterior part. It is fixed by muscles, as in the scorpion, and has a distinct muscular coat consisting of transverse strong fibres. Numerous

small vessels are given off from its sides, and two large trunks are seen to diverge from its anterior part.

In the crustaceous animals the vascular system arrives at the greatest development presented by this great division of the animal kingdom, both in the structure of the heart and in the extent of the arterial and venous distribution. The heart is attached, as in the other entomoid classes, and is generally a short muscular sac, situate in the middle of the dorsal part of the cephalo-thorax.

We observe the bluish white and highly fibrinous blood, in these crustaceous animals, returning by numerous veins (*Fig.* 170, *a b c*) from all parts of the body, which

Fig. 170

the cavity of the cephalo-thorax, and thence towards the middle of the back. In the middle of the back, where those trunks of the branchial veins terminate, there is a large muscular sac (*Fig.* 170, *g*) with thick parietes and internal muscular columns, which is the ventricle that propels the blood backwards by one great trunk (*Fig.* 170, *k*), and forwards by three great arteries (*Fig.* 170, *h i i*) in the decapods through the whole system. In the branchiopodous crustacea, however, the heart is only a lengthened dorsal vessel.

We observe in these large decapods that the heart is surrounded by a wide pericardium, which Treviranus and Strauss regarded as an auricle receiving the blood from the branchiæ, and that there are generally three great vessels (*Fig.* 170, *h i i*) given off from the fore part of the heart, which convey the blood forwards to the abdominal viscera, and to parts about the head; and the great posterior arterial trunk (*Fig.* 170, *k*) conveys it to the parts of the post-abdomen. So that we observe in the strong muscular sac (*Fig.* 170, *g*), enclosed in a pericardium, and provided with distinct valves, of these most elevated of the crustacea, the highest stage of development of the dorsal vessel of insects and worms, and beginning as a simple, thin, membranous, lengthened tube, in the larva condition of the insect, and in the lowest of the worms, which are only permanent larvæ, you have seen that dorsal vessel gradually becoming more and more concentrated, by its progressive march of development towards one point of the trunk, so as ultimately to construct this great fleshy ventricle of the crustacea, which receives the arterialized blood from the respiratory organs, and propels it by numerous arterial trunks through the whole system. And in tracing the march of development in the vascular system of the highest of these crustaceous animals, from the embryo state, we observe it passing through the same series of forms in its great centres, which we have seen in tracing its permanent conditions from the lowest of the helminthoid to the highest of the entomoid classes.

vessels at length are directed towards the base of the respiratory organs—the gills, where it is collected into a series of wide sinuses (*Fig.* 170, *d*) at the sides of the trunk. This venous blood is conveyed by the branchial arteries (*Fig.* 170, *e*) along the exterior margins of the gills, where it ramifies and subdivides over the innumerable minute laminæ that compose those numerous and complicated organs. From the extreme capillaries of those branchial trunks, it is collected by corresponding branchial veins (*Fig.* 170, *f*), containing the arterialized blood, which pass along the interior margins of the gills; those branchial veins convey the arterialized blood by numerous lateral trunks into

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Sir C. Bell's Discourses.—The following lecture (page 875) and the lecture immediately subjoining it (page 881), complete the course of lectures delivered by Sir C. Bell. The two present lectures are so intimately connected in subject and interest, that we have thought it altogether desirable to unite them in the same number of the journal, which we are enabled to do by publishing a supplemental sheet.

ANATOMY AND PHYSIOLOGY.

LECTURES

ILLUSTRATED BY THE

HUNTERIAN PREPARATIONS

IN THE MUSEUM OF THE ROYAL COLLEGE OF SURGEONS, LONDON.

Delivered at the College in 1833, *by*

SIR CHARLES BELL, K.G.H., F.R.S.

LECTURE XIV.

THE NERVOUS SYSTEM.

GENTLEMEN,—In looking back upon the subjects of this short course of lectures, we may find some that will assist us in our present more difficult task. You will remember that I stated it was necessary that we should mark the difference between animals, whether they belong to the vertebrata, or to the lower classes, and that we had occasion to remark that the vertebrata were peculiar in this respect, that they had a spine, and ribs connected with it, muscles to move those ribs, nerves to connect those muscles together, and a source of energy for those nerves in the medulla spinalis. We had occasion, then, to remark that animals of the higher class were totally different from those of the lower. Now this is an introduction to the subject on which I am about to speak, because the condition for action is connected with the condition of the whole frame.

Again, when we came to the muscles, we found, I might almost say innumerable muscles, under the direction of the will; but, perhaps, fifty muscles at least are drawn into one simultaneous action by the will; and this must be accounted for with a full understanding and acknowledgment that there is no relation existing directly between or amongst the muscles themselves, and, therefore, we must look for some means by which they are combined. Then we recollect that there is a class of muscles called flexors, and opposed to these, or contrasted with them, there are extensors. A relation is established between these, and that relation must be accounted for by reference to some third power, because a connexion is not common among themselves.

Then, again, when we had, as it were, accounted for the act of volition in the motion of the body, we had reason to conclude that the same muscles used in that motion were brought into a new combination, a combination which is withdrawn altogether from the influence of the mind, and the functions for which they were connected are secured by another source of action. So far we see that there must be different nerves, with different senses, which shall diverge, to proceed to and connect together the several muscles.

Now let me observe to you, that I conceive that physiologists of great merit amongst us have erred in going direct to their object when they made dissections of nerves, or experimented upon nerves, without sufficiently comprehending the use of the nerves; that is to say, the functions necessary to be performed through their means, or the combination which is necessary in the performance of the several functions of parts or organs. I think that some have lost themselves by going too direct to the object, instead of diligently studying the number of parts, for example, that must be associated in the single act of drawing breath, and the additional association of parts when the act of speech is engrafted upon that of respiration. How many muscles must be added to those employed in the single act of respiration before the voice can be called into exercise; and how many more before the voice is articulated in language! Or, again,—as we have been more lately considering,—how different the sensibility must be, and how different the arrangement of the muscles is, within the orbit of the eye! Is it reasonable to expect that we should account for the action or influence of the nerves which enter the orbit, if we have not previously diligently studied the functions which are to be there performed?

I do not know, then, that I can do better than recollect the manner in which I used to express myself, being convinced that in doing so, I excited my young friends to greater diligence as students, and afforded them some gratification.

I took up the subject in this way:—When the human body, for example, is preserved in spirits, and taken out for ten or twelve hours in a day for dissection, and the dissection is continued for a month or six weeks, what an extraordinary exhibition of nerves is presented! But the most remarkable thing is, that if we take another body, and dissect it with the same care, we shall find the same parts precisely. And if, again, you examine a third, every twig that was in the other two appears in this one. You have, in the first body, a little ganglion, a little knot, looking as if it were accidentally placed there; and when

you examine the second and third bodies, you still find it at the same spot, and having the same relations with other parts.

If you take up a nerve, say a little twig, which seems to be a mere individual part of a mesh, and look in the same direction in two or three bodies, you will find the same twig there also, in the same place, with the same relations, and preserving the same connexion.

Thus you see that the nerves are different altogether from the bloodvessels. The arteries all proceed from the heart, but they may take a devious course, the branch in one body going direct, in another going circuitously, or in a third you have two or three branches performing the one office. Nothing of this kind takes place in the nerves; the apparent irregularity in them is the effect of the imperfection of our own observations; they are perfectly regular; you cannot see a branch in one instance without seeing the same in another body; you cannot find the smallest twig bearing a relation to a particular nerve, but you will find it, if you go to other pieces of dissection, in every instance the same, and, therefore, our first impression that this is an accidental tissue is at once removed. There is nothing at all irregular or accidental in it. On the contrary, we find enough to prove that there must be some system, an exact arrangement of parts, to explain which puzzles us, and we want a key to the difficulty.

Exactly the same effect—one of disappointment on finding such regularity—strange as it may seem, occurs to the student, if he turn over the leaves, for example, of Bichat, Walther, Monro, Scarpa, and now we may add our own authority, Swan.

The first thing that strikes us now is, the extraordinary intricacy and difficulty of the subject, and we must, in some manner, endeavour to remove it, and to find the system which we are sure must exist in consequence of the same phenomena presenting themselves in every instance. We find that even down to the time of Dr. Baillie the same notion has prevailed, and that a very natural one, that the brain was the centre of the nervous system,—that the brain was the *officina spirituum*,—that something was there secreted, or provided, which went out by the tubera of the nerves, and was conveyed and gave life to every part of the body.

Now this is well enough so long as we take a superficial view of the nervous system, because it seems that the nervous power does thus diverge, and that the properties of life are cut off by the depletion of that particular nervous energy. But when you come to the personal examination of the body itself, then the facts are at total variance with the hypothesis. What is the meaning of one nerve crossing another without joining it? What is the meaning of one nerve crossing and joining another? You begin to discover that there must of necessity either be different sources of nervous energy, or different endowments leading through those nerves to different parts. It cannot be that the whole nervous power diverges from a single part of the brain, and thence passes out to the several parts of the body. Such a supposition is inconsistent with what is discovered in dissections.

In my endeavours to find some key, or some circumstance, which should direct me in this matter, I began by imagining that I could make something of the origin of the nerves by experimenting upon the brain. Accordingly I made small perforations in the skulls of animals, and passed a fine needle and thread through different parts of the brain, imagining that in that way I should inflame it, and produce a morbid influence on several parts of it. I then proposed to observe the effects, but was completely disappointed in the attempt. I could make nothing of my experiments, and could find no clue to unravel the intricacy. I next set to work to compare the roots of the nerves with their distribution; but what exceedingly perplexes one in such endeavours is, the opinion that the nerves are common nerves, just as we say there is common sensibility. I laboured to show you lately that there is no such thing as common sensibility; but even the great authority to which we have clung throughout the whole of this course—John Hunter—seems, by the catalogue, to say that there is a common sensibility. But I would deny that, and say instead, that there is a sensibility in the body, which differs in kind and degree, and is evidently appointed to regulate or influence every part in a due degree. But still there is a common nerve with common qualities, and there is no hope of extricating ourselves out of the difficulty. If we could but find an instance where one nerve has one quality, and another another quality, that would encourage me to persevere. I observed in Bichat's work this remarkable fact, that in opening a dog and making a semilunar gash, no pain was manifested; while in irritating the lumbar nerves, pain apparently was felt, and there was motion. This encourages us to go on, because it implies that sensibility is not of necessity the effect of nervous matter, but the nerves may have different

endowments. Then comes the question, can this be a common nerve? It is impossible to contradict what is universally believed. The nerve of the arm or a nerve of the body has a common quality. The whole profession cannot be mistaken in this. Have the nerves roots? Has nerve a common quality, or distinct qualities, because it has distinct roots? Are the distinct qualities the result of those roots having each a different qualification or endowment? If this idea once enter the mind of the anatomist, he remembers full well that every several nerve has two roots, two classes of roots; not individual fibres, but two classes of fibres, running longitudinally, arising in a line, and distinct in the manner and place of rising; distinct in structure; one with a ganglion and one without. When this fact strikes the mind, one naturally says,—It is a pity but what one could ascertain that these roots have different qualities.

As, then, I was careful enough to make experiments upon the living animal, and could bring nothing to light, I supposed that one of the roots was for sensation and the other for motion; that if I touched the root of sensation, the animal would experience pain; that if I touched the root of motion, no pain would be felt; I therefore took the less cruel method of destroying life, as regards sensibility and pain, and then opening the spinal marrow, and ascertaining what took place on touching with the point of a needle first one class of nerves, and then the other. I did so, and found, on touching the posterior roots of a nerve, that no motion took place in any part of the body, but upon penetrating deeper, and cutting the anterior roots, motion was produced, and the motion was produced as regularly as the needle went down touching the different anterior roots, just as distinct as the motion produced by touching the keys of a harpsichord. On touching the motory roots no effect is produced, and what is wanting is the knowledge of the simple fact, that the nerves are appropriated to different purposes. Anatomy supplies all the rest, and when once you are in possession of this fact, and you come to understand that sensibility and motion are not necessary consequences of nervous matter, you turn back to the arrangement of the nerves, to see under this new intelligence of their property what anatomy will do.

It was after arranging all this, and observing it, that at last I thought of a new field of inquiry, not at the roots, but in the distribution of the nerves. Now if this were to enter your minds as anatomists, would you begin to make experiments? I well recollect the old experiment of my master, and the account he gave of it; I mean Alexander Monro; you will see it in his beautiful work on the nervous system. "Here are the muscles of the face," he said; "here are two nerves going to those muscles, and for this purpose; Nature is more provident of the face than of other parts, and see here are two nerves going to the same part, lest one should be cut off and life altogether lost; that is to say, if they went together, or there was only one nerve, life would be cut off, but in order that that may not take place," he says, "the nerves come by different foramina, and by different roots to the same part." You will not wonder that this was an unsatisfactory view. At all events, here was a field for observation, for here are nerves continuing to run by a different course, and joining only in their fibres; and, after years of labour upon the anatomy of the nerves, you can conceive the pleasure of finding the results obtained from operating and experimenting. Here is a drawing, in which you see one nerve coming out, which I shall fully describe by-and-by, and there is another. It was pleasing to find, that on cutting the seventh pair, all motion was stopped, and that on cutting the fifth, all sensibility was destroyed, while motion remained. Again, then, you see it was made clear to demonstration, not merely to such a demonstration as a minute anatomist could follow, but obvious to all who possessed reason, that the nerves had different qualities, and that these resulted from the nature of their origin,—not their distribution, not their structure, but their relation to the centre of the nervous system.

But still, with all this, what is the meaning of the intricacy? For supposing that we were to trace out these nerves, and to find that they go off with great regularity from the brain and spine to every part of the body, with a double function—at all events, with a double distribution—and, therefore, were common nerves with common qualities, it would appear that the whole nervous system ought to be very simple, because the two qualities go out together, and the nerves are common. Then what is the meaning of the intricacy? Recollect the premises; remember what I stated to you before,—that the power of the muscles does not proceed in one uniform course from the sensorium, the moving instrument, nor that every sensation experienced in them goes directly back to the sensorium, but that a number of combinations, or acting parts, are associated in the simplest act of volition. We have further to remark, also, that, in many instances, these mus-

cles, instead of being directly subject to volition, are, purposely, and for benevolent purposes, withdrawn from its influence, and adapted to their particular functions independently of it. How? Certainly by means of nerves, by the relation and connexion of nerves; and we shall find, that in proportion to the intricacy of the relation of muscles to their several functions, so is the complication of the nerves supplying them.

There is another mode of proceeding in the investigation of this subject, and that is by tracing back our steps, and observing that the nervous system is accommodated to the variety of action which we have already noticed. Take the mollusca, and you will observe that the nerves are very simple in them. Now here is an animal possessing both sensation and motion, but without symmetry in its capsule. It is an oyster. Here is the section of a muscle which is an adductor, and which closes the shell, and here is a nerve going to the muscle, combining the fibres of the muscle together, and here is a point representing a little mass of nervous matter called a ganglion, and which is the source of motion in the muscle. Then we find a nerve going through the body and another ganglion, another nerve belonging to another muscle, which also is designed to close the shell. Then, again, we find a nerve which exhibits no regularity, running up the animal. Here all is irregularity; you see nothing that you can call a system. But you have arrived at this knowledge, that where a faculty is given to a part, that faculty is associated with a certain portion of nervous matter, and power diverges from it. If, again, you take the star-fish, for example, which belongs altogether to another class of animals, something like symmetry will be found in it, but the existence of symmetry will not explain what is intricate in the nervous system of the higher animals. You have the mouth, the œsophagus, the circulating blood, and the nerves going out to the several parts which require motion, and nerves that bestow motion and associate the parts together.

We take, again, the gasteropods. You remember that we spoke of animals as being in classes, of animals in the body of which a division existed, a sort of cortex, a firm integument, a firmness necessary to their motion and protection. But if the animal consisted of one regular homogeneous body, it could not move, and therefore it is divided and articulated in, and surrounded by, an external skeleton, the muscles being inserted into different segments, or circles, or annular parts, which, it is quite clear, must be the sources of motion. Now there is a source of motion in each division; each ring has a little brain; each ring has a little division of nerve going from it. Here there is something like symmetry? And why? Each division possesses its little ganglion, and from each ganglion there go off nerves to move the numerous muscles, but no one part can move by itself; there is a relation or union established between all the moving parts.

In the lobster you see this more perfect still. The plan of the nervous system is obvious. There is a series of ganglia a little raised; there are distinct series of eminences, with nerves going from them, having one line of connexion running through the whole, and imparting common sensibility to the animal—if I may now use the words in that sense—and common motion,—a concatenized motion, a combined motion.

Now in these simpler animals or lower animals, corresponding with their simplicity and regularity of form, there is a line of nervous matter connected with each ganglion, and in the higher animals there is the same, only that it is obscured by other organs of motion, other centres of sensation, and other more intricate branches.

Now if we take a creature lower in the scale than these, the earth-worm, and divide it, the posterior portion will remain writhing and twisting, but there is no associated action between the parts. The anterior part will move away. Labouring under the impression of pain in the posterior part, it will make its escape. Thus in the lowest of these animals you find, that small as the anterior ganglion is, yet it seems to be the superior. It is that which gives animation and consent to all the others. There, however, the anterior ganglion is acquiring a new property; new muscles are added to it, new nerves are given forth, and traced out to the new organs of sense. There is not a new sensibility to touch and pain, but appropriate nerves and ganglia, which ganglia run together into a little mass which we must call a brain.

There is still another very interesting mode of studying this subject. You know that some insects undergo metamorphoses. At one period of their existence they are hermaphrodites, and at another period they possess new organs and parts, wings and new muscles. If we are right, then, in supposing that the nervous system will always bear a relation to the parts to be moved, and to the variety of organs possessed, we have here an interesting occasion of witnessing that fact. The best

opportunity of doing so is to be found in the change which takes place in the scarabæus nasicornis. When that animal has undergone its metamorphosis, when at last the wings are expanded, and the muscles of the wing become active, new nerves are distributed in that direction, and the very remarkable fact is observable with your glass, that the nerves which led to the old muscles of the feet are withdrawn, and disappear altogether, and now there seems to be developed, coming into existence, as it were, altogether a new class of nerves, with fresh intricacies, for the muscles moving the wings do not exhibit the regularity of form which distinguishes the muscles of the feet. Here, then, is an instance of the whole nervous system changing its parts, and becoming curiously adapted to the new power required for moving.

We should now attend to the apparent resemblance in the human body to the line of nerves in the lower animals.

In the preparation before me, you see the sympathetic nerve running its whole course, and you would almost fall into the same opinion which others have entertained, that the sympathetic nerve is the ganglionic nerve, as it is called, of the lower animals. Strange it is that this opinion should be supported by such an authority as BICHAT, because he himself has taught us differently; he has taught us that when the ganglion of the sympathetic nerve was touched, no irritation was produced that was at all proportionate to touching one of the other ganglions, thus implying that the sympathetic nerve does not possess the quality of receiving and conveying impressions, or of producing impressions by means of other nerves. What, then, is obvious here? Why, that this nerve is known only by its negative qualities. It does not move parts, and it creates no sensation. How many demonstrations, I ask, were given of these nerves in former times? I remember well that I was taught to demonstrate the sympathetic nerve, beginning with a minute dissection of the bones at the base of the skull, marking its connexion, marking, after a day's laborious dissection, its connexion with the sixth and the second division of the fifth, tracing down the nerve along the artery, marking its coming from the jaw and communicating with the ninth and sympathetic, with the eighth and the cervical nerves,—observing its superior ganglion in the neck, the lower ganglion, and then the succession of ganglia running down to the body.

But there arose no thought in the mind of the physiologist at that time that this nerve was exceedingly small in the root, and that in this respect it was obviously distinct from the other nerves. The question arose whether it was going up, or, rather, coming down, and then the experiment was made of cutting the sympathetic in the neck; and it was conceived, in some minds, to be conclusive, that the effect of the division was inflammation of the eye. By-and-by, however, when the whole came to be fairly dissected, we were, as anatomists, relieved from the labour of trying to recollect the different relations of the sympathetic, because it came out that its relations were universal. At that time, too, anatomists were exceedingly curious about the phenomena dependent on its connexion with the fifth and the portio dura,—the seventh, and so forth. Yet it was obvious that it had as large a connexion with the nerves that went to the arm as with those of the head, and it was quite as obvious that it continued to the pelvis, and did not terminate there, but threw out fibres to the anterior crural nerve, and went down to the extremities of the toes. It is plainly, then, an universal nerve, a nerve extending all over the body, and further remarkable in possessing no sensibility. It was left for the physiologist to conceive that it was the nerve which least communicated with the mind, if at all connected therewith, or with the sensorium; that it was the nerve which united certain parts of the body into one; that it was performing all those secret operations of the economy which you know can be directed without the interference of the mind, such as digestion, assimilation, circulation, and all the functions which are necessary to the continuance of life; and there seems, even at this time of day, no reason to contest that conclusion.

But looking thus at the sympathetic nerve, we are left to account, as it were, for all its relations. Although it has obtained the character of accounting for all the sympathies of the body, it really does not account for one of them. These depend altogether on other relations.

Returning then to the spinal nerves, and what is seen upon opening the spinal canal, and observing the spinal chord and the nerves which proceed from it with such great regularity, we notice that each nerve has two distinct roots; one set coming from one column of the spinal marrow, and the other coming from the other column. Through the relation of these to the sensorium a man experiences all the influences of external agents, and has the various capacities of motion; that is to say, by their means he is subjected to the different influences which animate

him, and on which depend all the powers by which he moves and becomes an agent. This is enough to know until you recollect that these powers must be maintained by the operations of the animal economy, and for which operations you have not yet accounted.

Presenting the subject to ourselves in this manner, we say that man is thus constituted with a capability of receiving and acting upon impressions, of locomotion, and of every sort of agency; yet with all this he is but half made up, for he cannot breathe. Now suppose all the muscles of respiration had to be put into action by the effort of the will,—that man had to set himself to work those muscles; why you know full well that all the actions of volition are brought forth only very slowly, and by means of experience, until at last they become perfect. Can you say then that the function of respiration should be performed after the same manner that he moves a limb? No. That mode requires time, experience, and improvement, and nature, therefore, has provided differently for breathing. She has established an instinctive feeling, a new sense of motion, different sets of nerves for the performance of that function, and these nerves, under the influence of instinctive action, begin, the moment that life commences, to perform the purposes required of them, and without them it is quite clear that life could not continue.

This, then, introduces us to the subject of the respiratory nerves, and a very curious and interesting but difficult inquiry here meets us:—Is there any point in the nervous system which seems, more than another, to be the centre from which the nervous power diverges to those several muscles which are necessarily associated in the act of drawing breath?

Let us here draw on our recollection. If an animal be struck at one point of the head, it dies in an instant; it never draws breath again. The blow produces no pain, no struggling; but the act of respiration ceases, and such is the condition of the animal if that act be cut off at once. But if the spinal chord be divided at a certain distance below that point, motion and sensibility are cut off, while respiration continues. Even if the spine be fractured, and the column torn across below the origin of the phrenic, the man lies dead and powerless in his body and limbs, but the act of respiration is still perfect, because the nerves going off to the face, nostrils, mouth, and lips, to the sterno-cleido mastoideus, the muscles of the shoulder, and the muscles of the outside of the chest, are untouched at their roots, and continue their operation, though sense and motion be gone in the rest of the body.

In experiments in which the brain is cut off slice after slice,—though the seat of voluntary power is removed, the centre to which every sensation is referred is taken away, the source of all voluntary motion is destroyed,—yet the act of respiration continues. Proceed a little farther, and interfere with the source of the nerves, when, instantly, the respiration stops. This brings us to the roots of the nerves. We are thus like children playing at hide and seek; we know *something* about it, but not *all*. We know that we cannot be far from the part in which the source of motion is secreted. We have found that though a given part be destroyed, motion continues; if the part above be destroyed, it continues; but if the intervening portion be destroyed, it ceases in an instant.

We may next draw an illustration from an acephalous child. If a child be born without a medulla oblonga[illegible] born; but if it be born and have a medulla ob[illegible] and lives for some t[illegible] of respiration doe[illegible] brain; certainly [illegible] the spinal marro[illegible] point on which[illegible] find now that t[illegible] to the pharyn[illegible] the heart, the [illegible] comes from thi[illegible] nerves which g[illegible] the body, and w[illegible] respiration, come[illegible]

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Still, however, we find an extraordinary degree of complexity existing. The nerves on one side of the neck are complex, even in the lower animals, and they are exceedingly complex in the human body. What is the meaning of this complexity? Even upon the supposition that the nerves proceed from a common centre,—that nerves of one class go forth to produce motion and sensibility, and another to effect respiration,—how can we account for all this irregularity, until we remember that the grand function of respiration, of decarbonization of the blood,—the grand function which is necessary to be performed in order to perpetuate the other different functions of the living frame, is used for a different purpose than its characteristic one? I formerly had occasion to state that a variety of purposes was answered by one agent or point, and an opportunity is here given in the act of

respiration to show the performance, by the agents of respiration, of other functions, functions which are most important to the mind,—for instance, speech, without which philosophers acknowledge there can be no high cultivation of the mind, for speech is the very instrument of thought.

Well, then, only consider how many parts must be united and related one to another, before a man can utter even a simple sound, and how many more before out of simple sounds he can form an articulate language, and then you will not be surprised at the intricacy of the nerves of the neck, but you will proceed with increased anxiety to dissect each particular branch of them, in order to observe their various functions and the high and important results of each individual part. But there is a variety of proofs of the distinct nature of these common nerves—I mean the system of nerves—and one is the distinction which exists amongst those of which I spoke as separate nerves. There is nothing so remarkable as this, that the death of one nerve will occur before the death of another—that when all sensibility is gone, when all capacity of receiving impressions of any kind is, as it were, gone, the act of respiration still remains perfect. We may say, in fact, in one sense that these nerves are the last to die.

There is one thing more upon which I would ask you to reflect. It is a circumstance of high interest, and of much importance, I think, in pathology. You infer, and we are all apt to infer, perhaps with perfect correctness, that the eighth pair of nerves, and the muscles which they excite, are under the influence of the condition of the chest, and, undoubtedly, when we recollect the distribution of the eighth pair, such must appear to be the fact. Well, then, mark this, that when you have divided the spinal marrow (and through the spinal marrow there is no connexion with the lungs), when you have divided the eighth pair of nerves in the animal—the horse or the ass (and you remember that in those animals dividing the eighth pair is dividing the sympathetic)—there remains no connexion between the lungs, the heart, and the external respiratory parts. Yet they continue to act. They do not act as the limb of an animal would act if it were rapidly separated from the body and a galvanic influence were applied to it. Here there is a combination among all the muscles, and there is proof that the combination is in the body. What is the nature of the influence which the brain or the sensorium holds over them it is not easy to say; but we have proof that when the head is removed these muscles act,—are concentrated together,—and the union of these muscles, therefore, must be in the body.

Now, when we cut off the body, by the division of the spinal marrow, and have cut off the connexion with the lungs by the division of the eighth pair, the act of respiration still goes on, and from this I would infer that there is a source of motion in the nerve of respiration, or in the medulla oblongata, independent of the lungs, and independent altogether of the state of the parts within. The point, therefore, which I conceive it important for you to observe is this,—whether or not the morbid conditions of the function of respiration,—whether some of those sudden deaths in which a breath is not drawn again, where the features are left undisturbed, where there is not the least pain,—are at all to be accounted for by supposing them to be the result of an affection of the centre of the nervous system, as regards the great function of respiration, rather than an affection of the heart or lungs?

It is a subject of difficulty, but you will be excited to its examination by its importance.

LECTURE XV.

(CONCLUDING LECTURE.)

EPITOME OF THE NERVOUS SYSTEM.

Gentlemen,—Were I to follow my own inclinations, I should rather wish to dwell upon the eighth pair, because undoubtedly they present much that is novel. There is a great deal still to be determined regarding its origin, distribution, and functions, but the present course is so nearly at a close, that I must advance at once to a subject on which there is no variety of opinion, at least regarding facts, and make a comparison between the nerves going off from the encephalon, and those from the spinal marrow.

Even this would serve me to bring to your recollection, that on looking at the base of the brain, a remarkable difference is discernible between the nerves going off from that portion of the nervous system, and those which issue from the spinal marrow. In the spinal marrow you see each nerve with a regular double root, and one ganglion at one of the roots, and, as we have hitherto supposed, those are combined together and form what will still be, in a limited sense, a compound or common nerve. But when we look at those nerves which go off from the brain,

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We may next draw an illustration from an acephalous child. If a child be born without a medulla oblongata, it is dead born; but if it be born without a brain and have a medulla oblongata, it breathes and lives for some time. Thus the motion of respiration does not depend upon the brain; certainly it does not depend upon the spinal marrow; but we are near the point on which it does depend, and we find now that the nerve which goes down to the pharynx, the larynx, the trachea, the heart, the lungs, and the stomach, comes from this very point, and that those nerves which go to the external parts of the body, and which move in the act of respiration, come from the same place.

Does it not appear, then, that the experiments, or, as I may say, the analogies we draw,—all that we can deduce by the aid of reason, corresponds with the facts disclosed by the anatomy of the body?

Still, however, we find an extraordinary degree of complexity existing. The nerves on one side of the neck are complex, even in the lower animals, and they are exceedingly complex in the human body. What is the meaning of this complexity? Even upon the supposition that the nerves proceed from a common centre,—that nerves of one class go forth to produce motion and sensibility, and another to effect respiration,—how can we account for all this irregularity, until we remember that the grand function of respiration, of decarbonization of the blood,—the grand function which is necessary to be performed in order to perpetuate the other different functions of the living frame, is used for a different purpose than its characteristic one? I formerly had occasion to state that a variety of purposes was answered by one agent or point, and an opportunity is here given in the act of

respiration to show the performance, by the agents of respiration, of other functions, functions which are most important to the mind,—for instance, speech, without which philosophers acknowledge there can be no high cultivation of the mind, for speech is the very instrument of thought.

Well, then, only consider how many parts must be united and related one to another, before a man can utter even a simple sound, and how many more before out of simple sounds he can form an articulate language, and then you will not be surprised at the intricacy of the nerves of the neck, but you will proceed with increased anxiety to dissect each particular branch of them, in order to observe their various functions and the high and important results of each individual part. But there is a variety of proofs of the distinct nature of these common nerves—I mean the system of nerves—and one is the distinction which exists amongst those of which I spoke as separate nerves. There is nothing so remarkable as this, that the death of one nerve will occur before the death of another—that when all sensibility is gone, when all capacity of receiving impressions of any kind is, as it were, dead, the act of respiration still remains perfect. We may say, in fact, in one word, that these nerves are the last to die.

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Even this would serve me to bring to your recollection, that on looking at the base of the brain, a remarkable difference is discernible between the nerves going off from that portion of the nervous system, and those which issue from the spinal marrow. In the spinal marrow you see each nerve with a regular double root, and one ganglion at one of the roots, and, as we have hitherto supposed, those are combined together and form what will still be, in a limited sense, a compound or common nerve. But when we look at those nerves which go off from the brain,

we find ten nerves, all remarkably simple in their origin, as compared with those which proceed from the spinal marrow, so that the object of inquiry is,—What peculiarity is there in the distribution of the nerves, which will clear up this apparent discrepancy between the origin of the nerves of the brain and those of the spinal marrow?

You may recollect that six nerves are crowded together in the brain, and that two other nerves are described as going to the face. Now is there any correspondence in the distribution of the nerves that are crowded together, and the two which are distributed to the face?

I will first turn to the tenth in the head, because it has two roots, and rises exactly like a spinal nerve. It is one of the regular nerves, and Willis calls it the *suboccipital*; it is the tenth of the head, but in all its circumstances it is like a spinal nerve.

Still looking to the specimen before me, we find the fifth pair with a remarkable ganglion, and the only nerve of the whole nine which has a ganglion. Our attention is thus immediately fixed on this nerve, because we find that one root of a spinal nerve gives motion, and we have more than a suspicion that the other gives sensibility. Now can it be that this nerve gives sensibility? We shall find that the fifth goes to every part of the head, and hence the anatomist marks its relations to different parts, and says that there is no part without a twig of it. Does the sensibility of the head depend upon the fifth pair? Now comes the experiment which I noticed before, that by dividing a branch of the fifth, dividing what is esteemed a ganglionic nerve, we cut off sensation. Heretofore anatomists presumed that the object of the ganglion was to cut off sensation. They judged too hastily by comparison with the ganglion on the sympathetic, and imagined that the sympathetic nerve performed its functions without the cognizance of the will, through the intervention of the ganglia. Monro, in combating the idea that ganglia cut off sensation, says distinctly that this cannot be, because they were attached to nerves which he knew to supply muscles. "That ganglia," he observes, "do not serve to render our motions independent of our will, as an ingenious author has supposed, is evident without observing more than this, that all the branches of the fifth pair, and the posterior half of all the spinal nerves of the voluntary muscles, pass through ganglia." But then he was not aware of all those circumstances which you now see plainly before you. The motor nerve does not go through a ganglion; the ganglion belongs to the root of the nerve which bestows sensibility, and, therefore, when we have further proved, by direct experiment, that the fifth pair gives sensibility to the head, that the different branches of the fifth cut off no sensibility from the parts, we have arrived at a most important conclusion. I am not speaking of the olfactory nerves, the nerve of vision, and the nerve of hearing, but with the exception of the proper organs of the senses. Throughout the whole of the body, from the top of the head to the toe, the sensibility results from a series of nerves having ganglions upon them. So far from the ganglia being the means of cutting off sensation, it is clear that those nerves which have no ganglia are not nerves of sensation. I have been particularly desirous of avoiding all speculative notions, in order that I might not invalidate facts. Now, then, if we regard the subject comparatively, we shall find that there is a nerve in all animals corresponding with the fifth pair in the head of man. If you take animals of the molluscous class, you will find little rings of ganglia, and branches going out from them. In all those creatures which present a regular series of ganglionic nerves going through the body, there is still a particular nerve, with two roots and branches, around the œsophagus. In fishes all the processes which possess sensibility are supplied by the fifth nerve, and in the duck, which has its bill so much under water, there is,—corresponding with the new sensibility, or the new manner of sensibility,—an extraordinary development of, or supply of branches from, the fifth pair of nerves to the bill. The trunk of the elephant, which possesses a very fine sensibility, has a division of the fifth pair passing along the whole length of it to the very extremity. So in the feline tribe; the sensibility which the hare has in its whiskers results from a branch of the fifth pair going into them. Although we have thus made out by anatomy, by direct experiment, and by following up the subject in comparative anatomy, that the fifth pair is the source of sensibility in the head, and is the proper representative there of the spinal nerves, yet one is obliged to refer to authorities as well as dissections, for a confirmation of this. On doing so, we find that Wrisberg, Prochaska, Paletta, Sœmmering, and, not least of all, Santorini, had completely made out, before we could apply ourselves to this question of anatomy, that the fifth pair was a double nerve, and had its double root, one root with a ganglion, another passing free of the ganglion. In the spinal nerves there

are two roots. That which is for motion goes out free of the ganglion, and that which is for sensibility passes through the ganglion, and these often join together, and form a common nerve. If you dissect out the two ganglions in the dog, that of the fifth and that of a spinal nerve, you will see that there is a strict resemblance between them,—that the texture is the same, and that it is very different from the texture of the ganglion of the sympathetic.

Now, somebody had found that the fifth pair was a sensitive nerve, and a sensitive nerve only, by which you will see that were we to be guided by experiments only, we should be liable to delusion and mistake, and unless I had been well aware, by dissection and by examining authorities, that it was a double nerve, I should have been thrown out in my pursuit. Had the opinion been received by the profession that the fifth was merely a sensitive nerve, it would have led us into some confusion, from which I now hope we shall extricate ourselves, and therefore I am indebted, and we are all of us indebted, to Mr. John Shaw and his brother, Mr. Alexander Shaw, for a decisive experiment on the subject. A horse was knocked down, and a rapid dissection made to get to the fifth pair. On irritating the fifth pair at the root the jaw was immediately closed, thus proving that that division of the fifth pair which keeps clear of the ganglion, runs into the muscles of mastication.

Arriving at this conclusion by experiment, and by the inductions of anatomy, circumstances immediately crowded upon my recollection, and must crowd upon yours, to which I will allude. I recollect the effect of an abscess seated anterior to the tube of the ear, which destroyed all motion in the forehead. I recollect full well dividing the frontal branch of the fifth nerve, without seeing any effect produced. Conceiving that I must have missed the nerve, I brought the knife again and again across the notch, but still there was no diminution of motion. I now saw why in twenty other cases in which the operation for tic douloureux had been performed, it was permitted to be repeated, which, be assured, it would not have been if the effect of cutting across the fifth had been the same as cutting across the seventh, when the face is twisted and drawn to the opposite side with an unsightly distortion. Distortion in these cases has not taken place, and therefore it is an additional proof that on cutting across the fifth, we only cut across that branch which bestowed sensibility, and not that which bestowed motion.

Here is a drawing exhibiting a section of the head and the branching of the fifth pair. It is very pleasing to relieve ourselves from the difficulty of recollecting these parts, by saying at once that it goes universally to the skin, to the muscles, to the cellular membrane, to the external and internal parts, and to all the cavities of the head, and is the nerve which alone bestows common sensibility, if you will permit me again to use that word.

Now there is another case,—an instance of a formidable character, coming, as it were, in aid of the conclusion,—a particular disease, which was proved by dissection to commence in the fifth pair of nerves. The patient had excessive pain in the left side of the head and at the left side of the tongue, but at the same time that there was this excessive pain in the face, or pain referred to the face, there was a total insensibility of that side of the face. Thus, at the same time that the poor woman complained of pain too severe to be expressed in words, she had no sensibility. The next remarkable circumstance is, that with all this pain, with all this destruction of sensibility of the side of the face, there was no obstruction to motion; and I beg you particularly to notice, that with this power of motion in the features of the face, as I have expressed myself, the masseter and the temporal muscles were entirely wasted, a complete proof that this nerve supplies the motion of the jaw and the sensibility of the head. There was a tumour affecting the nerve at the root, and the progress of the disease was observed by the injury of the extreme branches. You know that when an impression is made on the trunk of a nerve, the sensation seems to be at the final distribution; and here this disease, while it was gradually destroying the nerve at the root, was disguised in a disease at its extremities; at the same time making such progress as to destroy the sensibility of the extremities, or cut off the sensation of the parts external to the sensorium. I dwell more particularly on this case, because other similar instances have come under my notice. I have a second case of the same kind in my note-book, and a member of the College has this season related to the Royal Society, another case exactly corresponding with these in all parts, the dissection proving the destruction of the root of the nerve, and the history of the case, as detailed by the patient, showing the destruction of the sensibility and motion of the muscles of the jaw, as the effect.

The next set of cases which illustrates the functions of this nerve, is derivable from instances in which disease takes

place in the ophthalmic branches of it, where also we have a series of distressing symptoms. The ophthalmic branch of the fifth pair being thus affected, the common sensibility of the external parts of the eye is gone, so that you may touch the eye with a feather, or with your finger, without producing sensation, and consequently without producing motion, in the eyes. Then comes the loss of motion in the eyelid, in the eyeball, and in the adductor. Why? Because, first encroaching on the fifth, and then on the third, the motor of the eye,—yet for a time the abductor is free, for the nerve supplying it is distinct; but, finally, that nerve, and consequently the adductor muscle, is involved in the disease, and ultimately the optic nerve itself, so that sitting down before such a patient, and gradually noticing the progress of the case, you are brought to an exact recollection of the anatomy and relation existing between the fifth, third, sixth, fourth, and optic nerve. You find these nerves cut off from their functions, the effect being the successive destruction first of one property in the eye and then of another.

The fifth pair, then, are an exact representation in the head of the spinal nerves in the body, and on tracing along the spinal cord up to a certain point, we find that the nerves are the same in their origin and in their distribution. On arriving, however, at the distribution to the face, the nerves are very remarkable, and a natural question to be asked is, whether the face is supplied with a double set of nerves. You would say, here, that the answer of my old preceptor, Monro, is by no means satisfactory. It was founded upon the old notion, that life and particular properties diverged from the brain through the nervous system to every part of the body. But now having found that the fifth pair bestow sensibility, the mistake of asserting that all the nerves are sensitive nerves is seen. The question returns, however, why do the nerves go apart in the face? For the same reason that they go differently in the body where the act of respiration is to be performed. They are the respiratory nerves of the face.

But some of you, perhaps, may feel an unwillingness to acknowledge the propriety of applying such a term to nerves of the face. Many highly philosophic men have done so before you. I remember that when these ideas first occurred to me, and I, undertaking to explain them, presented the seventh pair as the respiratory nerve of the face, an eminent gentleman said to me—"What has the face to do with respiration?" I entreated patience, however, in order that I might have the opportunity of explaining what was meant by the term "respiration." We are in the habit of regarding respiration too much as a chemical process,—as consisting of the decarbonization of blood merely, or the motion of the lungs; but it implies the motion of that which is necessary to the action of the lungs, and without which their motion would be attended with no effect. If I were selecting a horse for purchase, I should like him, first of all, to run a little on a course, that I might observe how the muscles of his ribs acted, see the condition of his neck, and I should not fail also to look at the condition of his nostrils. I remarked to the gentleman who spoke to me, that he would perhaps attend to what I was stating, if I could assure him beforehand that by selecting a small branch of the seventh nerve, and dividing it, I could stop the motion of the nostrils, without diminishing the sensibility of the part. Thus I endeavoured to bring him to a right understanding of the term "respiratory nerve." I meant, in thus speaking of it, to indicate that branch of the nervous system by which several parts were combined to effect one single operation or motion. Probably all of you have seen a man in a state of apoplexy; you have seen the effect of a want of action in the lips; that the lips fall before the breath like a valve. You have heard of, if you have not seen, the same relaxation of the velum pendulum palati, or the relaxation of the larynx, and if you will for a moment consider what is the condition of the respiration of a man who falls down in a fit of apoplexy, you will see the necessity of a nerve of communication between the main engine, the chest, and the tube through which the air passes. You will see that these being membranous tubes, if the lungs were inflated and the air drawn in, the tubes must collapse,—that, in short, exactly the same effects must take place as occur in apoplexy. But in order that the breath may be drawn freely, all these membranous tubes are in an instant expanded; so are the nostrils and the pharynx, and the little chink of the glottis is opened by a sympathetic action. All these tubes, then, must have a certain degree of dilatation to allow the air to pass freely through them, otherwise the increased respiratory action will be unavailing, or, worse than that, it will be attended with suffocation. Thus, if you see a horse breathing which has been run hard, you see that every corresponding part is in the same degree of extraordinary motion.

Again, it has been asked, and by most intelligent people,—and therefore you will

see that I am bound to suppose that there may be some here who will put the same question,—"What have the eyelids to do with respiration?" Here is a nerve, we will say the portio dura, running to the eyelids. We can understand why the nostrils must dilate in unison with the lungs, but what have the eyelids to do with respiration? Now I entreat you to mark the effect. When disease or division of this nerve takes place, the eyelids remain open, the effect of which is, that the muscle behind propels the eyeball forward. The tone and action of the orbicularis oculi of old authors being lost, the eye starts forward, like the eye of the hare, ever watchful, ever open. Now this is an unnatural and dangerous condition, and it is not only dangerous to the eye, by leaving it unwashed and unwiped, as it were, and liable to have matter collected upon it, but dangerous to the condition of the nerve within. Whilst you are in a fit of coughing—some of you may now perform the experiment if you choose (*much experimental coughing*)—or sneezing—that is an experiment not so readily performed —(*universal circulation of snuff-boxes*)—if you put your finger upon the eyelid you find at the moment of the action that the eyelid becomes as firm as a board. (*General coughing, sneezing, and elevation of fingers.*) The muscle of the eyelid is then in action, and its firmness guards the eye, not merely from external violence, but from internal injury. The *succedaneous* act of respiration in coughing or sneezing drives the blood into the organ of sight, and the blood might injure it did not nature, kind nurse, take care to support it by pressure from without. Whenever, therefore, a violent act of respiration is performed, this delicate organ is guarded by its orbicular muscle. You know that when you sneeze in the dark, a corruscation of light appears before the eyes. Now reflect on this, and you will probably come to the conclusion which others have formed, that the seeming sparkling of fire arises from the forcing of blood into the retina. But this is not the fact. It arises from the sudden and violent action of the eyelids to protect the eyes against the very circumstance which you suppose really does take place, namely, the forcing of blood into the eye. If you perform this experiment by taking snuff in a dark room, you will find sparks before the eye when you sneeze; but keep the eye open, and you will not find any such effect produced, because the contraction of the eyelids upon the eyeball is prevented. You may produce the same effect by the sudden action of the eyes. It is certainly not properly a branch going to the eye that produces respiration; but how shall we now consider the question? Return back, and recollect that there is one grand function which keeps the whole frame of the vertebrated animals in action,—respiration. Advantage is taken of this to effect something more than a chemical change in the blood. It ministers in a very important manner towards producing other capacities in the lungs. At the same time recollect, as worthy of your contemplation in admiring the works of creative power, that delicate as are the organs connected with respiration, and agitated as the whole frame is by violent coughing, yet we are not left without a protection against that agitation. If you follow this subject up, you will arrive at most curious conclusions, and, also, plainly see what I meant by saying that nature was a kind nurse. She guards you against harm by a variety of little contrivances which your ingenuity would never suggest.

I have said that this nerve must go to the mouth, because the mouth must be associated with other parts in the act of respiration. Permit me briefly to allude to this in the following manner, because one does not like to have papers torn from us, or pronounced false, when they are not so. In my first paper published in the *Transactions of the Royal Society*, I gave to the fifth pair the term of "the nerve of sensibility and mastication." MAGENDIE, when making experiments on this subject in the horse, found, as he conceived, that I was incorrect in this, and that instead of the motion of the lips in mastication belonging to the fifth pair it belonged to the seventh, and that in cutting the seventh he not merely stopped the act of respiration, or those acts which are combined with the respiration of the animal, but also all motion of the part —*all motion*. Now see how apt we are to be deceived by experiments. This experiment was likely to throw us all into confusion again, for it seemed to prove that the seventh nerve was the muscular, and the fifth the sensitive nerve. But two mistakes were involved in this supposition; first, the inference that the seventh was the only muscular nerve; and secondly, that the fifth was purely a sensitive nerve. This, then, threw me back again upon the authorities which I have mentioned, and I returned to the investigation of the roots of the nerve. One of them having a ganglion and the root passing by the ganglion, I had only to trace out those branches as before, when I again found that those which passed the ganglion went, not as was supposed, to the lip, but to the temporal, masseter, and pterygoid muscles, and to all the muscles that wrought on the

jaw. Now having been so far correct, I think it was rather a pleasant thing to be forced back to one's former course, and find that the original remark was true. Anatomy is, after all, the best possible guide in such matters.

This explains what I alluded to when speaking of disease of the root of the fifth pair,—that the muscles of the nostrils, the muscles of the forehead, and the muscles of the face, remained of their usual strength and fulness, and in fine activity, but that the temporal and masseter muscles wasted away; and you must presume that the pterygoid muscles also wasted away. It is a remarkable thing that while disease of the fifth pair is thus destroying sensibility in the side of the head, and all the muscles which move the jaw, the jaw continues to be moved by the muscles of the other side. It is this circumstance that deceives one. There is in such a case this remarkable effect,—a wasting of the muscles of the jaw, while the other muscles of the face continue entire; but the effect is very different indeed when the portio dura is affected.

With regard to the portio dura or respiratory nerve. What is the meaning of the act of respiration being cut off from the fifth? I stated to you before, that if you could suppose a man constituted entirely of a nervous system which was capable of receiving all sorts of impressions and of using every sort of agency, you would still have to examine into the means of his performing those vital operations which are necessary to his existence. You have, then, in addition to the nervous system alluded to, certain nerves possessing a new origin, and having sources or property of motion, whereby the act of respiration is performed to the extent which we have seen. These nerves being established for the necessary purposes of respiration, what is the meaning of the intricacy of nervous structure which exists in the neck? Why, nerves are required to come down which shall pass to the glottis, the larynx, and the pharynx, which shall carry their action to the larynx as well as to the muscles which are correctly enumerated as muscles of respiration. The necessity for them is made palpable when we reflect on the simultaneous acts which must take place before we can speak.

When we look to the glosso-pharyngeal nerve, the par vagum, and the accessory nerve, all arising from one column, one cannot but notice that the portio dura comes from a posterior part, and is directly associated with the nerve which draws these parts into the act of respiration. Why, then, does it go circuitously round the face, and by a different passage, and through different foramina? It is to allow of those several associations which are necessary to respiration in the face, the throat, and the neck; for we shall find that when the portio dura is affected externally, there is a loss of action in the face, and that when it is affected nearer to the root, there is a loss not only of the action of the face, but of a certain action of the throat. I would have you notice this particularly. You have what is called the portio dura of the face, and if you are anxious to discover at what point an injury of the nerve is situated, whether it is near the base of the brain or in the course of the nerve, you have only to look into the throat, when if you find the uvula is lying all on one side, you know that it has partaken of the paralysis of the face, and, therefore, that the affection of the nerve is near its root.

But I am anticipating too much. I should first endeavour to show that the portio dura is the nerve of motion to the face and eyelids, and is the respiratory nerve, and then illustrate the position by cases. Allow me then to go back and say that on cutting the portio dura all respiratory action ceases, and at the instant with it, all expression. Most remarkable phenomena are thus exhibited in this experiment. There is highly excited action on one side of the face, while on the side on which the nerve is divided, the placidity of death prevails.

The first case to which I shall allude in illustration of this is, that of a man who sustained a fracture of the temporal bone. After blood had been poured copiously from the ear, there came forth a serous secretion, and the tube of the ear and the whole side of the face were paralyzed. His eye remained open, his lips were without action, and he died. On examining the part, it was found that the rent went through the temporal bone, and had torn the nerve as it passed through that bone. The next example is that of a man who was tossed by a bull, the horn of the animal having entered the ear, and torn the root of the nerve, more externally, with the same effect. There are two cases now in the *Middlesex Hospital*, one of which I have not much noticed, but the narrative given of it so exactly corresponds with other statements of the same kind, that I have a full belief of the facts detailed. There is a fracture of the jaw, which has torn the pes anserinus, or portio dura, just as it comes out anterior to the head. There is paralysis of the whole of the side of the face, and the motion of the lips and nostrils is gone; but now I observe that motion of the eyelids also is gone, and with the failure of motion of

the eyelids we have another opportunity of observing the necessity of the combined action of those parts. The eyelids remain open, and all the fine apparatus which we have been admiring in the eye is thrown out of use. The tears are not brought over the eye, the eyelid does not wash the cornea, the cornea remains exposed, opacity has taken place in it, and blindness is the consequence. Thus you have an opportunity of observing in a direct manner that the eye may retain its power of vision, by means of the optic nerve, while, having lost its sensibility, it loses, as it were, the principle of activity, and destruction comes upon it. Or, again, while the sensibility to light and impression remains in the eye, the lids are destroyed by the loss of the nerve, their motion is impeded, the cornea becomes dry, gets into an unnatural condition, ulceration takes place, and, finally, a loss of sense in the organ is the consequence.

I hope you understand me. MAGENDIE says, that when a branch of the portio dura going to the lip was cut across, the motion was entirely gone, but he and others were too hasty in concluding that that was the only nerve of motion, and in this they declare their ignorance of the main principles which should have guided them—that the fifth pair is a double nerve, a nerve of sensibility and of motion, a nerve of motion going to the muscles of the jaw for mastication,—that the portio dura is not a simple nerve of motion, but a nerve which supplies the means of associating the face, the lips, the nostrils, and the eyelids, in the act of respiration,—that it is not a simple nerve of motion, but is combined with the other nerves of respiration, and is, so far, entitled to the name of *respiratory* nerve.

It is difficult to touch upon these subjects and bring them to anything like a natural conclusion, where there are so many curious phenomena to be observed,—some inexplicable, and some still remaining open for investigation. But upon the whole let me hope that I have given you a short but clear epitome of the whole matter,—a brief account of the nervous system; and let me recommend it to your attention. I hope I have proceeded far enough to entice you on in its study. You will find it to be a part of anatomy which is most intimately connected with the functions of life, and therefore most likely to furnish you with new means of observing the phenomena of life and the symptoms of disease.

On concluding this short course of lectures, I must acknowledge that there is great difficulty in doing so at this point. I believe that no one who has preceded me in this place could hesitate to acknowledge that there is great difficulty in stopping here, from two causes; first, from our being left very much without any direction as to the subject-matter that should be delivered to you, though I fain hope that that will be remedied; and, secondly, —another difficulty, which is still greater — from the nature of the audience and the manner in which it is composed. If I were to raise my voice, and try to give reasons to my young friends who are studying for the profession, for every point that is propounded, I should encroach upon the time of those who are older, and that would be sinful as a trespass upon them. I have, therefore, both to accommodate myself sometimes to younger men, and to address those who know how to keep pace with me in all I say; and, finally, even if I ought to address myself to the senior portion of my auditory, still there would be a difficulty in regard to the language which I should employ; for on some occasions we have had with us in this theatre the first scholars in Europe. Let me, however, at least not forget to thank you all for the good-nature and patience with which you have heard me throughout. (*Applause.*)

CLINICAL OBSERVATIONS

ON

VARIOUS DISEASES,

BY

MR. WARDROP.

DISORDERS OF THE RETINA OCCASIONED BY DISTURBED ACTION OF THE HEART.

I AM not aware that CORVISART, LAENNEC, BERTRAND, or any author who has treated of the diseases of the heart and of the various symptomatic disorders which accompany them, has alluded to the affection of the retina which I am about to describe, although it is by no means rare.

That there are ocular spectra caused by a disturbed action of the heart, is a fact the knowledge of which is of great practical importance, since the channel in which remedies should be applied is thereby pointed out, and thus, instead of treating only a *symptom*, our curative means can be directed to the organ primarily affected,—the diseased heart. An acquaintance with this affection of the retina I have also found eminently useful by indicating the existence of a disorder of the heart in cases where suspicion of that affection had never previously existed.

It is well known to pathologists that *spectra* or "false images" which appear before the eyes, depend upon different causes,—arising in some instances from diseases of the eyeball,—in some from diseases of the parts surrounding the globe,—in others from diseases within the cranium,—while in others, again, they are symptomatic of disturbance in the digestive organs. The ocular spectra, however, which I am about to mention do not owe their origin to any of these causes, but are produced by an unequal distribution of blood to the brain, in consequence of some change in the action of the heart.

This affection of the retina is to be carefully distinguished from those appearances before the eyes which often accompany "congestion of the brain," because those two affections require very different treatment. In the former we have to endeavour to restore and regulate the heart's action. In the latter, the quantity of blood within the head must be diminished, and I need scarcely add that the same observation applies to those spectra which arise from a disturbed state of the digestive apparatus.

Case 1.—I was consulted by Dr. PAUL respecting a young lady who suffered severely from ocular spectra, the images before both eyes materially interfering with the functions of those organs. She had no symptom of congestion in the brain, nor of disturbance of the digestive organs; neither could the spectra be referred to any changes in the eyes themselves. On feeling her pulse, my attention was directed to a peculiar *thrill* in it, which I had often remarked to be connected with disease in the heart; and on examining the state of that organ, I found its impulse greatly increased, accompanied by other symptoms of hypertrophy. It now, for the first time, occurred to me that a change in the circulation of the blood through the brain, as well as an increase in the quantity of the sanguineous fluid, might produce spectra, it being very common in apoplectic patients, or those suffering from congestion in the brain, to have various images floating before their eyes.

Under this impression, our first endeavour was to relieve the disease of the heart, and for this purpose a few leeches were applied on the chest, and antimonials were given to moderate the heart's action. These produced the best effects, and after being a few times repeated, the impulse of the heart was so much diminished, that nothing remained but an irritable state of that organ, which was speedily subdued by the exhibition of the sulphate of iron. Whilst this amendment was going on in the state of the heart, it was gratifying to observe a corresponding improvement taking place in the affection of the visual organs, the images floating before the eyes gradually becoming less and less distinct, and ultimately vanishing when the natural action of the heart had been restored. After the lapse of several years, she informed me by letter that she continued "perfectly free from all defect of vision."

Case 2.—I was consulted by a young lady who complained that various images were constantly floating before her eyes, and that she wanted power to read distinctly,—objects appearing to her as though covered by a gauze. She had also a "swimming in the head," coldness in the feet, a whizzing noise in both ears, dyspnœa, and cough.

Suspecting that these spectra, and the symptoms by which they were accompanied, were connected with a disturbed circulation, I examined the heart, and found its impulse much increased, and an agitated pulse. She stated that, two years before, she felt, after severe mental affliction, palpitation of the heart, and that about six months afterwards the imperfection in her vision became manifest.

I recommended four leeches to be applied to the region of the heart, a dose of James's powder to be taken every night, the pediluvium to be used every evening, and the bowels to be regulated. In six days the symptoms were not much mitigated. Four leeches were now applied to one foot, and the James's powder and laxatives were directed to be continued. These produced the best effects; the heart's action was gradually subdued, the function of respiration became natural, the affection of the eyes and the swimming in the head diminished, and I then gave her an opening pill, and soda, with a bitter infusion. She called on me six months after this was ordered, when I found that the affection of the eyes, of the head, and of the chest, had completely left her, and that her general health was better than it had been since the commencement of her illness.

Remarks.—Nothing is more deserving of attention, when examining different instances of disturbed circulation caused by changes in the heart's action or structure, than the differences observable in the effects which those disturbances produce, not only on the respiratory and digestive organs, but more especially on the functions of the brain and nerves. The influence of diseases of the heart on

the functions of the nervous system has been long well known to pathologists. This, however, only makes it the more extraordinary that the real cause of the affection of the retina, which I have now described, should have escaped so many acute observers.

That this affection, as well as that of the nerve of hearing, subsequently to be noticed, should have been overlooked by those who have lately investigated the diseases of the heart, can only be explained by the fact, that whilst they have been endeavouring to discriminate the diseases by investigating the different sounds and movements of the heart, they have neglected to attend to the general assemblage of symptoms accompanying them.

The *facies propria* so vividly depicted by Corvisart, and the various alterations which the faculties of the mind sometimes undergo, when the heart is diseased, as well as the particular feelings of "swimming in the head,"—difficulty in sustaining the balance of the body, &c.—all sufficiently point out how much the functions of the brain are influenced by changes in the velocity or momentum of the blood, or by irregularities in its distribution within the head.

It still remains an interesting subject of inquiry to discover what are the changes in the circulation, or what are the particular changes in the structure of the heart, which produce affections of particular parts of the nervous system; how it is that disease of the heart is accompanied in one person, by ocular spectra,—in another, by constant sounds in the ears,—and in a third, by more or less disturbance of the intellectual powers.

COMMUNICATION FROM

DR. AYRE, OF HULL,

ON THE REMEDIAL POWERS OF

CALOMEL

(*In Small and Frequent Doses*)

IN THE TREATMENT OF

MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—From the numerous communications which I have sent to your journal on the subject, there can be few of its readers who are uninformed of my having employed and recommended small and very frequent doses of calomel in the treatment of the malignant cholera; and that whilst there were many who adopted and commended the treatment, there were others who neglected and decried it. To the objections, such as they were, and founded as they were on no trial of the remedy, but on reasons purely hypothetical, I never deemed it necessary to reply; for having once performed what I regarded as my duty to the public and to the profession, by communicating, first through your journal, and subsequently in a separate publication, the method of treatment which I pursued, with the reasons and results of it, I resolved to leave to those who should hereafter have to treat the disease, the final decision on the merits of the remedy; believing, too, that, with the departure of the disease from Europe, had departed also all interest with the public in its treatment. Circumstances, however, are changed. The disease has reappeared amongst us, and in all its former horrors; and I should now feel it to be criminal in me, as the author of that practice, if I longer delayed the lending of my feeble aid to the laudable efforts of others, to inform the profession of its value; and especially after the example given by Dr. Peacock of Darlington, whom I knew but by name and reputation, who, as an earnest of his entire conviction of the specific power of calomel in small doses, to cure the disease in its blue or collapsed stage, has offered, if any two respectable practitioners will give to it a full and patient trial, to present to any public charity a sum of money for every case of the disease in which it should be found to fail.

It is not my intention to detain your readers with any restatement of the principles which guided me to the choice and employment of small and frequent doses of calomel, nor to enter into any argument upon the objections which are urged against their use; for the objections, as I have already stated, are based on mere hypotheses regarding the nature of the disease itself, and the operation of the medicine upon it, and have been raised by those who have never given it a trial, or, trying it, have combined it with other means, and thereby marred its effect. By some its sole action is assumed to be stimulant; and because stimulants in every degree and form have proved unavailing, it has been concluded that calomel must be so likewise. By others it is held as equally certain, that it can only act beneficially by its being absorbed into the system, and that, therefore, the largest dose should be most efficient; but that as the largest doses have proved useless as remedies, so must be found the smaller ones. Others, again, whilst acquiescing in the

assumption that the medicine can alone act by being first absorbed, have denied that it thereby becomes a remedy; for persons are known to have been attacked and to have perished by the disease at the very time when they were under salivation. The conclusions, therefore, which the persons holding these opinions, and the multitudes of others who have given heed to them, have been, that calomel is of no virtue in this disease; and as all other means have confessedly failed, and there is no substitute offered for calomel, that there is no *remedy* for the disease; and writers have not hesitated to affirm, that patients very rarely recover from a state of collapse, and never when they have been pulseless in it; and such is their confidence acquired from their *own* experience, that they avow, with little either of propriety or delicacy, an entire discredit of those who have affirmed the contrary.

To such reasonings and statements as the foregoing, the only suitable reply is *facts*, resolvable by the reader into arguments; and I shall now proceed to detail a series of these, and so amply authenticated as to prove, if I mistake not, to the satisfaction of the most incredulous, that it is *not* by being absorbed that calomel acts upon the disease, and that it is practicable, when given in small and frequent doses, and *uncombined* with other means, to restore the patient though he be *pulseless*; and to cure, even with comparative facility, and without salivation, and without being *succeeded by fever*, the blue or collapsed stage, as characterized by the *cold tongue*, the *sunken eye*, the *cold and shrivelled hands*, the *small* and *rapid pulse*, the *choleric hoarse voice*, the *vehement cramps*, *suppressed secretion of the kidneys*, and the *colourless* and *copious discharges*, with the other signs of the disease under its ordinary forms of severity, and before that intensest state of it comes on, when the vital powers are overwhelmed, and the torpor of the stomach and system is induced, which renders them wholly insensible to the action of the most powerful agents.

The facts which I shall now give, will be such as have had in every case more than one medical witness, and in many of them several, and to whom I confidently appeal for their truth; and to preclude the possibility of all suspicion of making a selection of particular cases to serve a purpose, and what might appear to be taken out of a large list of failures, I will limit the quotation of cases to *all* the *first* cases which were attended by *all* the practitioners, including myself, whom I have *known* to employ this treatment, and with whom, in company with others, I have been in personal communication, and have either visited the cases, or become immediately informed of the result of them. Proceeding, then, upon this rule,

The *first* patient, who in the blue or collapsed stage was treated by the method of small and frequent doses of calomel, was a woman in Sunderland (Mary Thornton), who was resigned to my care for that purpose by Dr. Clanny and Mr. Embleton of the Dispensary, who witnessed her state at the period when the treatment commenced, and watched her through the course of it. She was in collapse when first seen, and having been bled to a few ounces, became almost immediately pulseless at the wrists, and continued so upwards of two hours. The only treatment besides the bleeding, consisted in two grains of calomel every ten minutes during several hours, with a minute quantity of laudanum. She was wholly recovered in a few days.

The *first* case that appeared in this town, and that was treated by this method, was under the care of the medical officers of the dispensary, who invited their brethren to visit it. The man fainted on the floor on the first assault of the disease, and immediately fell into collapse, but was early seen, and in a few days was out of doors and recovered.

The *first* patient attended by me in this place was a sweep's wife of drunken habits, whom I saw along with Mr. Sharpe and his assistant Mr. Cooper. This woman was in the most entire collapse, and had been pulseless the preceding hour, and continued so to the end nearly of the one during which we remained with her. Mr. Cooper continued his attendance during most of the night, and gave her the pills himself. She was fully restored to health before the end of the second week.

The *first* case which Mr. Sharpe took the sole charge of, and that was treated on this method, was a woman in North Street, and whom I saw with him at the time when she was becoming convalescent, but with some remains left of the sunken state of the eye and the hoarse voice. She recovered in four days.

After continuing a few months, the disease left this town, but reappeared in a brief but severe outbreak of it in the following year.

The *first* case that then occurred, fell under the care of Mr. Bolton, of Beverley, and of his partner Mr. Williams. It was one of full collapse, and was treated in the same method as the cases already given, and very soon recovered.

The *first* case which I saw at this return of the disease in that year was in a man

(Knight) in New George Street, whom I visited with Mr. Sheckleton, late of the Cholera Hospital. In ten minutes after our seeing him, and before any means were used, he became *pulseless*, and continued so more than an hour; yet he was out of his collapse in less than twelve hours, and on the following day sate in a lower room, with his family, at the breakfast-table,—walked out on the fourth day, and on the fifth one rambled as far as South-end, a distance of more than half a mile from his house.

In the early part of last month the disease again visited us, and still continues here.

The *first* person whom I saw in it, on this occasion, was a man in factory side, North Street (Dandy). This patient was in a collapse, of the usual severity; and the two medical gentlemen (Mr. Munton and Mr. Marston), whom I took to see him, avowed their belief that he would not recover. It was, however, in pursuance of a resolution which I had long formed of having competent medical witnesses to every future case which I might attend, that I had called then to see this patient, and I stated to them that, from experience in the power of the treatment, I was confident that the man would recover, and that I had called them, not so much to see the case, as to witness the efficacy of its remedy. The same means as in the other cases were used here, the *one remedy* of single-grain doses of calomel given every five or ten minutes, and continued perseveringly through many hours, with a widening of the intervals of their exhibition as the symptoms subsided. The patient was sufficiently well on the *following day* to dress himself, and to go down-stairs; on the *fourth* day he walked out, and on the *fifteenth* day he walked a distance of sixteen miles into the country. Besides the two gentlemen named above, who saw this case, it was subsequently seen by Mr. Henesy and Mr. Jenkins, both of whom likewise became converts to the method of treatment, and employed it in the cases which next came under their care.

The *first* case which Mr. Marston had, and which was so treated, was a boy (Hinchcliff), 13 years of age, in Collier Street. He was seen by me and others whilst in his state of entire collapse. He recovered in a few days.

The *first* case attended by Mr. Jenkins was a man in Portland Street (Ingham). He was seen by me with Mr. J. whilst in the most profound collapse, and a report was brought a few hours afterwards, and during the night, that he was dying. His pulse, when it could be felt, was 130. After recovering from the collapse, he became affected with fever, was bled twice, and leeched, and fully recovered in about ten days.

The *first* case which came under Mr. Henesy's care, that was treated *exclusively* upon this plan, was a man who was in a profound collapse, and was seen also by Mr. Marston, and at a later period by me. His pulse in the right wrist was extinct. He was bled on coming out of collapse, and in a few days was well.

I might add to these some other "first" cases, as the *first* male adult whom I saw was with Mr. Sharpe and Mr. Cooper, in Newton's Court, Rachel Street, and who was in collapse, and recovered in three days.

The *first* child (Souter), in Church Street, when I also attended with the same gentlemen, and who was pulseless for upwards of an hour, was so far recovered on the fourth day as to be playing in the street before the door.

The *first* patient whom I attended in the hospital (Susanna Curry) was admitted in a profound collapse, and who, after being nearly recovered from it, was thrown back again into it by something improper which she had secretly eaten; yet she was fully restored to health before the expiration of the week in which she became an inmate.

To these cases I may now add a summary of the results of the treatment employed on upwards of two hundred patients. In the first great outbreak of the disease, there were but two hundred and six patients who were exclusively treated by calomel; for out of my whole number of two hundred and nineteen patients, thirteen of them did not take any medicine. Of the two hundred and six persons, there were thirty whose cases proved fatal, either by the treatment being delayed until the vital powers became irrecoverably overwhelmed, or by neglect in taking the medicine, from the difficulties and privations of every kind to which the poor are subjected. The remainder, to the number of one hundred and seventy-six, recovered, and a good proportion of them from a state of the most entire collapse.

Neither in the first, the second, nor in the present outbreak of the disease, did I ever see a case of failure where a reasonable opportunity was afforded for exhibiting the remedy, or a reasonable attention employed in the giving of it; and if there be cases of such malignity, and I can believe from great inanition and other causes there may be such, as to resist even in the incipient state of collapse, the remedial, and I must add, the specific,

powers of this medicine, when given in small doses, and frequently and perseveringly renewed, I can state that I have not met with them, nor have I met with any cases in the premonitory stage which, when treated by this method, have ever farther advanced.

To the facts then afforded by the foregoing series of cases I would now appeal, as evidence of the truth of the position which I have set up, that calomel given in small doses is a remedy for this disease. Upon the nature and force of this evidence it is not necessary for me farther to dwell. The cases which have been given were *all* of collapse, and some of them to a profound degree. They were all *first* cases, and, therefore, *unpicked*, and were *all* the FIRST cases of *all* the individuals with whom I had personal communication, and whose cases, with one exception, I had seen. *All* the patients, with two or three exceptions, escaped the consecutive fever, and the constitutional effects from the medicine. They were *all* treated by the *same* remedy, and they *all* recovered.

To the evidence here given, I hoped to have been able to add the authorized report of a commission of six or more persons, half of them being medical, and which I solicited the members of the corporate body of this town to organize and superintend,—the duties of the persons forming such commission to consist in visiting and making minutes of the *precise* state of each case at the period of my visit, and to report the results of the treatment of it. Had my request been complied with, I doubt not the result would have been to establish in the public mind the same conviction of the value of this remedy, which has been so long established in my own, and which almost every hour during the present period tends but the more fully to confirm. As it is, I am willing to hope that the brief and imperfect notice which I have here again given of the remedial power of calomel, will not be without its effect; and by leading to its more general adoption, procure for the community that is visited by the disease, a very large diminution of its agonizing sufferings and losses from it.

I am, Sir, yours, &c.,

JOSEPH AYRE, M.D.

Hull, Sept. 6th, 1834.

*** The former communications of Dr. Ayre on this subject will be found in Vol. II., 1831-32, at page 43 (No. 450), page 105 (No. 452), page 271 (No. 457), page 458 (No. 463), and page 615 (No. 468); and in Vol. I., 1832-33, at page 18 (No. 474), and page 637 (No. 489).

CASES OF

MALIGNANT CHOLERA

SUCCESSFULLY TREATED WITH

EMETICS OF COMMON SALT.

By ISAIAH BURROWS, *Surgeon to the Devonport and Stonehouse Public Dispensary and Eye Infirmary, and late Surgeon to the Cholera Hospital, Stonehouse.*

THE perusal of a letter from Mr. Beaman on the treatment of malignant cholera with the chloride of soda, has afforded me infinite satisfaction, as corroborative of the views I have for a long period entertained respecting this highly-important and direful malady, and now establishing the opinion I have expressed to my immediate medical friends, that it was the only remedy the administration of which could really be justified.

The reasons which led me to employ it are the following:—During the period in which I held the office of surgeon to the Cholera Hospital at Stonehouse, in 1832, I experienced many conflicting feelings on witnessing the recovery of numerous cases of cholera without the use of any internal remedy, and others, again, in which the patients died, notwithstanding the use of every plan that had been submitted to the profession, and thus, after passing through this perilous duty in safety, my mind was a perfect chaos, and I could not resolve upon any plan of treatment with satisfaction or confidence. When, then, a few cases appeared in the autumn of last year, I was almost in despair, and being thus led to review the notes I had made of my treatment on its first visit, I found this important fact presenting itself,—that in *every case that recovered* I had employed *common salt*, avowedly, it is true, as an *emetic*. In many instances it did not act in this way, and when retained was much more beneficial to the patient. This circumstance suggested its employment in the three following cases:—

Case 1.—11th Oct. 1833. Mr. A——, ætat. 56, of intemperate habits, having experienced severe vomiting and purging, sent for his medical attendant, who found him extremely frightened and weak, and his voice reduced to a whisper. Emetics were administered, followed by large doses of calomel, in conjunction with other remedies, but without avail. He was also bled. The symptoms advanced with rapidity, and at 9 p. m. the pulse was not to

be felt at the wrist. I was then requested to attend, having found that very prompt treatment had been employed. I related the above circumstance, and we accordingly administered two tablespoonfuls of salt, dissolved in a pint of lukewarm water; this the patient greedily swallowed, and retained the whole for twenty minutes, when he vomited a little, and discharged about a teacupful. A change quite unexpected followed; there was a return of circulation to the wrists, his countenance brightened, the skin and tongue became warm, his voice more audible, and at the end of three-quarters of an hour, only one discharge had taken place, small in quantity and of a dark colour. Reaction was established to the entire satisfaction of his medical man, and a decided improvement was manifest to the friend, and to the patient himself. Calomel and diaphoretics were persevered in during the night. At our visit on the following morning, we found that frequent bilious discharges had taken place from the bowels, and he had passed a little urine. Mercurial action was by this time set up. He continued in a favourable state during the day, and took broth frequently. On visiting him in the evening my astonishment was great on finding that he had suddenly relapsed into as bad a condition as on my first visit, and he died comatose that night.

Case 2.—The same evening on which this patient, whose case I have given above, died, I was requested by a marine, since deceased, to visit his wife, living in George Street, named Trethewy, æt. 22, attended by the assistant-surgeon, under whose care she had been the whole day. He had administered to her effervescing draughts and calomel, and as instantly were they rejected, and from the opinion he had given that she would die, the friends sent for me, regardless of his displeasure. I found her in the condition so often described, and as the malady could not be mistaken, the symptoms need not be related. Having but the day before seen the good effects from the salt and water, I dissolved two tablespoonfuls in a pint of lukewarm water, which I administered to her in two portions. The first dose was immediately rejected. She then drank the other without a moment's delay, and this was retained. Before, however, I gave her any, she refused to take it in consequence of the violent vomiting that was excited whenever she attempted to swallow, but to her husband's joy it afforded a relief quite unexpected. I remained with her one hour, and while there she had no return of the vomiting, and passed only one small evacuation; her countenance changed from the ghastly hue, which is so characteristic of the disease, to its natural colour. The pulse beat distinctly at the wrist, and the whole surface of the body was warm. I then directed that a small quantity of tea should be given, as the thirst had rather increased than diminished. This sensation, however, was different. The request made for cold water is generally expressed for the purpose of allaying the sensations of heat and oppression at the precordia, but now it was needed merely to moisten the mouth and fauces. Having obtained for her fresh blankets, and applied external heat, she was considerably better. I replenished the jug with salt and water in the same proportions, and desired that a *wineglassful* should be given to her every hour. This was punctually attended to, and, I found, even wished for by the patient oftener, from the relief it afforded. At 6 a.m. the following morning she was visited by her medical attendant, and the delight he felt was expressed, in no measured terms, to the husband and friends. I saw her at 9 a.m., and found complete reaction, almost amounting to fever; bilious discharges and urine had been passed. Not till this visit was I aware that her medical man had not been informed of my attendance, although it was upon a contrary condition alone that I consented to see her; however, I must have the testimony of that gentleman himself, that the case was a most severe one, and that up to the last moment he saw her it had proceeded uncontrolled, notwithstanding his judicious measures. She experienced many fluctuations in consequence of the illness of her husband. She was afterwards bled and other remedies were employed to allay the feverish symptoms, and eventually she recovered.

This case was reported to the commandant of marines, and her husband was not permitted to enter the barracks, but remained with her, and acted as her nurse. Two days after, the poor fellow showed symptoms of the disease. His anxiety for his wife's safety was very great; he had attended her assiduously in a small close room, which, together with the fear of his medical officer's displeasure, altogether increased the symptoms. He was immediately admitted into the Royal Naval Hospital, where he died of malignant cholera.

Case 3.—On the 9th of November I was called upon to visit a young man named Horswill, æt. 19, dissipated in his habits and dirty in person, who had been at the fair in Plymouth on the Wednesday evening previous, and had drunk of different kinds of spirits. The following day he had several loose evacuations, which in the course of the night increased rapidly, at-

tended with violent cramps, and on the 11th he could not move without perfect exhaustion. Notwithstanding the urgency of the symptoms the friends did not send until 2 p.m., when the disease was completely developed, and it was apparent to them that he was suffering from cholera, having seen some of his own friends die of this disease. Here was another case fairly developed, and it gave me a full opportunity of trying the chloride of soda. I need not recapitulate the plan I pursued, as it was precisely similar to that employed in the last case, and attended with like results. At the end of a fortnight he was quite well and walking.

Not considering, however, that these cases alone would be sufficient to confirm its use, and none others coming under my notice, the disease not being general in the town, I have never given them publicity, more particularly as the salt was universally known as a remedy, or rather as an efficient emetic. With regard to the *modus operandi* of the salt, I cannot for a moment believe that the vomiting is its only effect; for if we examine the cases above detailed, it will be found that the solution *was retained* by the stomach, and the more frequently it was repeated, the more apparent and permanent was its *influence*. From Mr. Beaman's own statement it will be seen, that unless it be followed up, and the solution is quickly repeated, a relapse ensues, and thus far we agree, as may be seen by referring to the first case. I think, Sir, that the communication of Mr. B. is one of the most important that has been laid before the profession, and it affords me much pleasure to confirm his statement, and adding my testimony to a free use of this valuable remedy.

Fearing, however, to extend this letter to an undue length, I will merely state my view of the way in which it acts, and the reasons which induced me to prescribe it in the manner mentioned in the last two cases; and first, I did so from the simple and plain fact that common salt being mixed with black blood immediately changes it from black to red. Secondly, because in the analysis of the blood which is found in the veins of persons dying from cholera, an absence of those constituents appears which are necessary to health and to produce secretions; and, thirdly, because in the evacuations which come from the stomach and bowels, a very large proportion of the essential salts of the blood is found, and among them is chloride of soda in a large quantity: consequently, if this be true, I ask, will the effort of vomiting alone impart to the whole mass of blood such a change as that which is seen to take place after the use of common salt? If vomiting alone has such an influence, we possess equally powerful emetics as this.

The principle of its action, then, I contend to be as follows: On the introduction of a solution of common salt into the stomach, its first and immediate effect is, that of an astringent. It then becomes absorbed, and immediate combination with the blood in the veins of the mesentery follows. This is now rendered fluid, passes on to the liver through the veins, assists in the formation of bile, which is now freely secreted, and passing into the stomach is either thrown off by that viscus, or going into the intestines is there discharged, after passing through the liver. If it be not expended there, it is then conveyed by the vena cava to the right side of the heart, combining with its contents, then passing through the heart and lungs, and thence affecting the circulation, and causing the alteration which is so visible after its employment, until its influence is lost in the general mass. Continue then to supply the system at regular intervals with this fluid, until the secretion of urine and the other functions are established, and the patient is rendered capable of receiving nourishment, which is soon felt, and you can with safety discontinue it.

Whether these ideas be correct or not, such are the effects following the use of this salt (if *any follow at all*), and I contend that it is not unreasonable to employ a remedy, which though it do induce an action, not altogether natural, yet if it be capable of giving to the blood a change nearly allied to the condition of health, gains for us an important advantage, and prepares the system for the reception of remedies to perfect the cure.

Thus, then, whether a trial be given to it in the way now proposed, I hesitate not to affirm, that it is the only remedy that has my confidence, or that I would allow to be employed, if unfortunately attacked with cholera myself. If, however, the facts which Mr. B. has brought forward, or the points which I have advanced, require further proof than the mere trial before any and every member of the profession, I confess my inability to produce conviction. Sophistry or argument alone I shall not have recourse to on the present occasion, but simply keep in view this grand axiom, that Medicine is a science of observation,—one that can only be perfected and raised beyond the suspicion of empiricism, by following the dictates of nature, and the evidence of our senses, discarding all theory unless borne out by reason, pathology, and facts.

Emma-place, Stonehouse,
Sept. 1, 1834.

OBJECTIONS TO RELIANCE ON SALT EMETICS IN ALL STAGES OF MALIGNANT CHOLERA.

To the Editor of THE LANCET.

SIR,—I hope the great importance of the subject will be a sufficient excuse for my again addressing you. In the last number of THE LANCET I find various communications on the treatment of the worst forms of cholera with salt emetics; and if the blaze of cures performed with that remedy had been shaded a little with an account of some cases in which it must inevitably fail, I do not know that I should have troubled you with this communication.

Soon after the disease broke out in England, I met Dr. Pidduck in consultation, and from the high testimony he bore to the use of salt as an emetic, I determined to give the remedy a fair trial, the result of which has been, that in twelve consecutive cases in which salt emetics were freely administered, where the operation was *most complete*, and the remedies were repeated two, three, and four times, and even oftener, after the cases became hopeless, under my own immediate inspection, nine died in the cold stage, and three ran into a lingering consecutive fever and died. I could have accounted for the discrepancy between my cases and the eleven consecutive cases of recovery treated by Mr. Beaman, if that gentleman had not stated the treatment to be applicable to "cholera in its *worst* form," and implying that his cases were in that condition of the disease.

I sent you a short account of some cases too late for insertion in last week's LANCET; and I again repeat, that although I know of no better remedy than a salt emetic in the *worst form* of this disease, I have not the least faith in such remedy; for I have seen it fail, most decidedly fail: nor have I observed, as Mr. Beaman states, "that on the following day the patient generally passes a small fecal evacuation, containing bile." On the contrary, I have usually observed that no water is passed during the first 48 hours, sometimes much longer, and that while gruelly motions continued passing generally two, three, or four days.

In justice to the gentlemen who attend at the St. Giles's Cholera Hospital, and who are a little aspersed by Dr. Todd in THE LANCET of last week, I take leave to say, that if the cases admitted there are such as I have usually seen in cholera hospitals and poor-houses,—aged, diseased, and labouring under the "worst form of the disease,"—much credit is due to those gentlemen in being able to save two patients out of fifteen. The pain of witnessing the death of so many infirm beings, without the power of affording relief, is surely quite sufficient without an accusation from a medical brother that in all probability lives were lost from an obstinate adherence to preconceived opinions, or a more culpable and guilty determination not to avail themselves of a simple and efficacious remedy to which their attention had long ago been directed. Let those who cure cholera when it has seized on an aged, worn-out, abused, constitution (as I will venture to say thirteen out of fifteen are, of the patients admitted into St. Giles's Cholera Hospital), who is seen for the first time in the blue, pulseless, frigid state, and who will be found to have laboured forty-eight hours under a violent attack of premonitory diarrhœa, with salt emetics, give publicity to the details of half-a-dozen cases in the pages of your journal. If medical men would oftener communicate the unsuccessful results of different remedies, many a rock, which others look to as a secure resting-place, would be avoided, and much valuable practical information would be recorded.

I am, sir, your faithful servant,

FRANCIS GODRICH.

Little Chelsea, Sept. 5th, 1834.

PRODUCTION OF CHOLERA BY RICE.

To the Editor of THE LANCET.

SIR,—The following facts, which were this morning communicated to me by Mrs. Smythe, I beg leave to send for publication in THE LANCET.

Mrs. Smythe, a lady of high respectability, living in the Strand, communicated to me on September the 6th, 1834, the following facts:—In the summer of 1832, half an hour after partaking of rice soup, which was composed of rice, water, and turnip, she was seized with most violent pains of the stomach and bowels, attended by all the symptoms of cholera. Another lady, who was in the house at the same time, likewise partook of the soup, and she was also affected precisely in a similar manner. Both patients recovered in consequence of prompt medical assistance. The quantity of soup swallowed by Mrs. Smythe did not exceed half a pint.

About a fortnight afterwards, Mrs. Smythe, not suspecting the rice to have been the cause of her sickness, again partook of rice soup, made of the same ingredients, and was a second time affected with symptoms of the same description; and since that time, under any circumstances, she has not made use of *rice* for food.

Mrs. Smythe has authorized me to say, that she is convinced it was the rice which made her ill, and that the other lady, who is since dead, was of the same opinion. Mrs. Smythe has also authorized me to state, that she is ready to declare the above facts and this conviction, before any person, and at any time. She ate the rice in London. The grain was broken and small, and had the appearance of having been exposed to water.

The description of grain alluded to by the lady above named, is at this moment exposed for sale throughout London at the rate of twopence per pound. A statement of the above facts, certified with her name, is in my possession, and can be seen by any of your readers who may be desirous of examining them. I beg to remain, Sir, your most obedient servant,

R. TYTLER, M.D.

London, September 6th, 1834.

PHRENOLOGICAL SOCIETY OF PARIS.

President, M. ANDRAL.

Annual Meeting, August 22, 1834.

HYDROCEPHALUS—CRIMINALS—BENEVOLENT PERSONS—ORGAN OF RAPE—MUSICAL TALENT—HEAD OF NAPOLEON.

THE expectation of a discourse, *ex cathedrâ*, on the head of Napoleon Bonaparte, assembled a numerous collection of phrenologists at the annual meeting of the Society at the *Hotel de Ville*. Unfortunately, M. Froissac, who was charged with the duty, was prevented from appearing. M. Dumoutier, who undertook to supply his place, had not sufficient time to make the researches necessary for a complete investigation of the subject. The meeting, however, on the whole, was so interesting as to induce us to lay a report of it before our readers.

The PRESIDENT (the celebrated M. Andral) opened the meeting by a short review of what had already been done in phrenology, and pointed out what still remained to be done, in order that many principles laid down by the science should obtain the authority of facts. As to the basis of phrenology, that is to say, the relation which exists between the configuration of the skull and the different moral and intellectual faculties of man, M. Andral showed that it was supported by so many strong presumptions as nearly to merit the rank of a certainty.

M. CASIMIR BROUSSAIS, as secretary of the Society, was charged with the duty of giving an account of the labours of the Society since the last public meeting. He commenced by observing, that the time when a doctrine required the greatest number of proofs, was that when its enemies showed themselves most active in attacking it. During the year which had just passed, the Society had collected a certain number of facts which served to confirm it in the belief of the opinions it had always advocated. After a few other observations, M. BROUSSAIS proceeded to pass in review about 20 heads which had been examined phrenologically, and exhibited plaster casts of each. The first head exhibited was that of a child eleven years of age, affected with hydrocephalus. This disease does not of necessity exclude all intelligence; but in order that the individual affected should preserve his mental faculties, it is indispensable that the volume of the head assume an increased development, proportionate to the quantity of fluid contained in the skull: by this means the effects of pressure are counteracted, and the functions of the brain, though in general more or less altered, may remain with some degree of activity for a considerable time. But in the case to which M. Broussais directed the attention of the meeting it was quite different; the skull in fact was two or three times smaller than natural, instead of being enlarged: hence the substance of the brain was reduced to a mere nothing, and the child, as a necessary consequence, was a complete idiot. M. Broussais next showed two brains taken from the dog species; one the brain of a spaniel bitch which was remarkable for her intelligence, the other taken from one of her pups, the only one of a whole litter which was totally inapt to receive any education. The speaker pointed out the striking difference of proportion between the two brains, and particularly the absence of *educability* in the grown pup, while the organ was very prominent in the mother. Passing to an examination of the heads of individuals whose lives had been stained by crime, &c., M. Broussais indicated the incontestable predominance of those regions allotted by phrenologists to the passions and instincts, at the expense of those where the intellectual faculties and sentiments are situated. Thus, in the head of Lemaine (who lately assas-

sinated the servant of Madame Dupuytren), he drew the attention of the audience to the enormous magnitude of the occipital region, and the length of the bi-temporal diameter. In the skull of Lemaine, the organs of *self-esteem*, *love of approbation*, and *justice*, are small. The murderer, however, was not without some share of benevolence; he asserted, with the utmost constancy, the innocence of Gilliard, accused as an accomplice, who was afterwards acquitted. The organ of *veneration* is sufficiently marked on the cast, and it is to be observed that he demanded the succours of religion at his last hour with much earnestness. The next cast exhibited was taken from the head of a young man still alive, 21 years of age, detained at the *Bicetre*. At eight years of age he commenced to steal from his parents; at nine he robbed his neighbours; and at ten, as he himself confessed, he conceived the project of murdering his aunt. The animal organs are very predominant in this cast; the forehead is retiring, and the superior region of the skull very narrow. He is a distinguished thief, and is stimulated to remarkable deeds by his *self-esteem*. This organ, which is very developed, has often excited him to break the loom at which he worked in the prison, to get rid of orders which he considered as tyrannical.—The heads next examined by the secretary were those of two suicides—one twenty years of age, an ironmonger; the other, aged thirty years, the chief of an establishment at the *Messageries Royales*. The first blew out his brains because his sister was unhappy in domestic affairs; the second drowned himself because his wife was a drunkard. In both these heads the organ of *love of life* is very little pronounced: *benevolence* predominates, as well as *courage* and *destruction*. Another individual, in whom the organ of *love of approbation* is great, is Thouvenin, the bookbinder. This man frequently ruined himself to satisfy his *amour propre*, and the passion of his trade by which he was so strongly possessed. There was something analogous to this in the organization of the musician Choron, who has just died. Preoccupied solely by his love for music, Choron neglected everything else: one day he met in the street a child who sung a popular air in a manner that pleased him; he brought the infant immediately home and installed him in his house. His wife, less enthusiastic than the musician, demanded where he was to get money to support this new guest. "Calculating soul (*ame venale*), said the artist, "while I think of a tenor, you think of your soup." The head of Choron does not present a great development of *ideality*, but we can distinguish the organs of *veneration* and *perseverance*; these explain his laborious efforts to become master of an art which he had not studied when young, and the almost exclusive taste he showed for religious music.—The attention of the meeting was now drawn by M. Broussais to two heads in which *courage* and *benevolence* were well marked. One of them, Henin, is well known for his devoted attempts to save the passengers from the shipwreck of the *Amphitrite*, at Boulogne. The second, an innkeeper at Villette, is commonly called "the saviour," from the number of persons he has rescued from drowning. The last individual to whom M. Broussais referred, presented the organ of *benevolence* in an excessive degree, so much so as almost to render the upper region of the forehead deformed. The cast was taken from the head of a young negro named Eustache, remarkable for numerous acts of benevolence, and the development of the cerebral organ corresponded with that virtue. Though possessed of a large fortune, he had completely dissipated it in gifts made without reflection or judgment; if *circumspection* and *firmness* had existed to counterbalance his predominant sentiment, we should have had a model of physical and moral beauty in the head of this unfortunate youth. After a short episode, in which M. Broussais attacked with warmth the enemies of phrenology,

Dr. VOISIN gave an account of a visit which he made in the year 1828, to the galleries at Toulon, under the authority of the Minister of the Marine. His sole object in this visit was to establish the existence of the organ of *rape*. The number of individuals confined amounted to 372, and he had to select amongst them 22 who had been condemned for the crime above mentioned. Having examined the posterior region of the head in all, he selected from the ranks 22 individuals, and on referring to the registers it was found that amongst the 22 individuals so selected 13 had actually been condemned for rape; the remaining nine had been incarcerated for different crimes, but they had been marked in the registry as requiring a special *surveillance* in regard to their morals.

M. FOSSATI followed, and delivered some interesting observations on musical talent, which depends on an organ situated above the external angle of the eye, and produces the square forehead so remarkable in celebrated musicians. When we would wish to designate a musician of high taste, &c., we are in the habit of saying "He has an exquisite ear," but this manner of appreciation is quite erroneous.

When Beethoven placed himself before a piano, his fingers ran along the notes, but never struck them; that would have been useless, as he had completely lost the sense of hearing; he was perfectly conscious of what he executed, through the internal organ of the brain, although the instrument remained as much without sound, as the player himself was deaf. The organ of the *measurement of time* is as necessary to a good musician as that "of the relation of tones," and singers have especial need of the organ of *construction*. The mask of Weber, analysed by M. Fossati, presented a remarkable development of the inferior and middle parts of the forehead, whence this composer was essentially predisposed to labour, and the study of the masters in the art. Although Litz has, according to M. Fossati, some analogy with Weber, yet he feared this young artist, with all his talent, was not capable of producing any thing to be compared with works of a higher order. If to the organs which refer specially to music be added a predominance of the spirit of criticism, this mixture gives rise to musical censors, as the speaker showed by several examples.—The skull of M. Paer, another musician and composer, presents a strong disposition to poetry, and a great development of *imitation*, and hence his music is eminently dramatic. As to Rossini, his enormous head seems to unite all the organs, all the qualities necessary to form a perfect musician of the highest class. The lateral development of his forehead explains how he has been able to make such an immense progress in instrumentation, and the sense of language, also very well marked, shows how he has composed to French words without ever injuring the prosody.

The discourse of M. Froissac was now expected with some little anxiety, and we were not slightly disappointed to hear that for some reason or other he was unable to attend. We were, however, occupied in lieu thereof, by examining the Emperor Napoleon's hat, the "very hat" which he wore at Waterloo. It was brought by a gentleman, who permitted the company to examine it at their leisure, and those who tried it on, which some did, declared their conviction that Bonaparte's head was much larger than it is generally represented to have been. In place of M. Froissac's discourse,

M. Dumoutier analysed the mask made by Dr. Automarchi, immediately after the death of Napoleon, and deductions were drawn by him in a sense quite opposite to those contained in a commentary lately published on the subject. The mask taken by Dr. Automarchi is not quite complete; it gives the face, forehead, and upper part of the head, as far as the organs of *firmness* and *veneration*. Spurzheim admitted thirty-five organs on each side of the head, and since his time two additional ones have been discovered. M. Dumoutier passed in review these several organs in the head of Napoleon, and drew attention to such as presented any remarkable degree of development. He particularly distinguished those organs which enable the individual to seize and judge of "dimensions, form, space; those of number and classification; that which enables one to remember places, relative situation, and events." The organs of *judgment, intelligence*, properly so called, and *comparison*, were extremely developed; that of *causation* feeble. On the summit of the head, the organs of *benevolence* and *veneration* are well developed; and that of *poetry* is very considerable. In the inferior region the *love of possession* is very visible, as also the *desire to dissemble*. On the contrary, the organs by which a man is drawn towards life &c. are very small. In conclusion, M. Dumoutier remarked, that, on the whole, the head of Napoleon gave indications of a high genius, and in a physical point of view was a model of beauty.

P. H. Green, M.B., Corresponding Member of the Society.

ROYAL ACADEMY OF MEDICINE, PARIS,

September 1st, 1834.

The Royal Academy of Medicine received the report of the Council of Health for the year 1830. We select the following particulars from the chapter dedicated to the mortality of Paris:—

The disease most fatal to the inhabitants of the French capital is phthisis pulmonalis, which destroyed 1148 males, and 1448 females. The period of life at which consumption was observed to prevail most, is between the ages of ten and forty years.

It is a generally received opinion, that *pulmonary consumption* is less prevalent in warm climates than under a cold and moist atmosphere, but it is well ascertained that the disease is as prevalent and fatal in France as in England or Ireland; it may also be remarked, that the deaths amongst the females was one-quarter more than amongst the males, and this proportion was still higher for the years 1827-8-9.

The next disease, in regard to the number of deaths produced, is *pulmonary catarrh*, which may be considered as the

consumption of old persons. The mortality was 995 males, and 1132 females.

Aneurysm of the heart appears to be a frequent disease in Paris; the report gives 194 cases of death in the male, and 396 in the female. This disproportion of aneurysms affecting the arch of the aorta (for this, doubtless, is expressed by the report under the term aneurysms of the heart) is remarkable, because it is a fact generally known that spontaneous aneurysms are much more frequent in the male than the female, at least for the great arterial trunks. Thus, JOHN HUNTER is reported to have said, that he never saw an example of true aneurysm in the female. In sixty-three cases reported by HODGSON, we find fifty-six belonging to the male, and seven only to the female. The proportion lately established by M. LISFRANC is still higher. In 154 cases (aneurysms of the arch are not included here) he found 141 males, and 13 females. Whence then arises this strange difference? The council ascribe the frequency of aneurysm of the heart in females, to the injurious pressure of the corset on the parietes of the chest.

Scirrhus, alas, is more frequent in the female than the male; the deaths were, 104 males and 419 females. It is easy to account for this difference anatomically. The presence in the female of two glands which do not exist in the male, exposes the weak sex to a disease almost peculiarly confined to the glandular system.

After pulmonary catarrh and consumption, the diseases most fatal are *enteritis* and *pulmonary inflammations*. Thus the deaths arising from gastritis were, 840 males, 1108 females; from enteritis, 1171 males, 1103 females; and these affections have prevailed most between three months and seven years of age. This latter observation confirms one which we have had frequent occasion to make while studying the diseases of children; derangement of the gastro-intestinal canal, so common in children, very frequently terminates in gastro-enterite of a chronic nature, and the child is cut off by an insidious disease, which is very apt to escape the notice of a physician not accustomed to the maladies of children.

Convulsions prevail most from the period of birth up to five years; the proportion of deaths in this disease was, 664 boys, and 746 girls.

Inflammation of the lungs does not appear to affect either sex in preference. Males 863, females 872.

Apoplexy 542 males, 424 females, nearly all from the ages of 30 and 50, and upwards.

Finally, *peritonitis* destroyed 125 males, and 409 females from 20 to 35 years of age. The consequences of parturition readily explain this latter difference.

ROYAL ACADEMY OF SCIENCES, PARIS.

PRESENCE OF MIASMATA IN THE ATMOSPHERE.

THE important question of the possibility of ascertaining chemically the presence of miasmatous matter in the air, has lately occupied the attention of this Academy. We remember, not long ago, to have heard M. MAGENDIE assert "that the presence of a highly putrid animal matter could be established in the atmosphere surrounding the bed of a patient labouring under hospital or yellow fever, by the application of cold." This, we believe, is a doctrine not generally held or accredited in England; we, therefore, take the opportunity of analyzing a paper read by M. BOUSSINGAULT before the Academy, on the 4th August last, and entitled "Memoir on the possibility of proving the existence of miasmata, and an hydrogenated principle in the air."

Amongst the many questions, says the author, connected with the chemical history of the air, there are few more important than that which has for its object the discovery of the cause producing the insalubrity of the air. The deleterious principle is so intimately mixed (or in so small quantity) with the air we breathe, that it escapes all endiometric means of research, yet it possesses the most energetic properties.

The most frequent cause of atmospheric insalubrity depends on the development of a certain principle which constantly takes place wherever dead vegetable matter is exposed to heat and moisture; it is peculiar to warm and marshy localities, particularly where fresh and salt water are mixed together.

It also manifests itself with great energy in ground lately cleared, as long as the vegetable debris is left to rot.

In all marshy countries the hygeienic means recommended by the inhabitants are the same; they advise you not to expose yourself to the dew which falls immediately after sunset, and it is everywhere remarked that a difference of level, often very slight, is enough to exempt the inhabitants from any danger.

From these and many other facts, it is concluded, that vegetable matter being decomposed under the influence of heat and moisture, produces miasmata; and also that the unhealthy air is more heavy than the healthy; and, finally, that it is deposited with the dew which falls abundantly in warm countries. Taking these data as his guide, MOSCATI undertook to condense the dew in order to seek the deleterious principle. His experiments were made about the rivers of Tuscany. The water which he collected, in sufficient abundance, on a cold body, was at first liquid, but soon deposited flocci, presenting the characters of animalized matter.

In 1812, M. RIGAUD DE LILLE made some analogous experiments in the marshes of Languedoc, but with a different apparatus, and obtained a liquid which putrified very readily, containing flocci of a matter having nitrogen for its base, and giving with nitrate of silver a precipitate which passed quickly to a purple colour. He attempted to ascertain, by experiments made on animals, the action of this liquid, and explained by it the cause of certain epidemics.

In 1819, M. BOUSSINGAULT, who happened to be in the department of Ain, remarked that sulphuric acid placed in the neighbourhood of a marsh, &c., became quickly black, while at a little distance the change of colour was very slow. At this period the whole country was filled with hemp-dikes, and fever was very prevalent. It struck him that the fever and change of colour in the acid depended on the same cause, viz. the presence of an organic matter in the air, which was carbonized by the sulphuric acid. He made, in consequence, numerous experiments to ascertain the value of this means of detecting miasmata in the air.

At a later period, he renewed the same experiment, in an unhealthy part of America (Valencia), and saw that the sulphuric acid became quite black in twelve hours; but the numerous insects which fill the air in tropical climates, compelled him to abandon this test as uncertain.

In 1829, he resided at Carthage, when the wind brings miasmatous vapours from the neighbouring valley, and with them fever, &c.

He now modified his process. After sunset, two watch-glasses were placed on a table in the middle of a marsh, one contained some distilled warm water; the other glass, its temperature lowering by the effect of radiation, was soon covered with dew, while the warm one evidently could not condense any moisture. A drop of sulphuric acid being placed in each glass, and the whole being evaporated over a lamp, a trace of carbonized matter was always found on the cold glass, while the other remained perfectly clean.

These experiments were continued for several nights, until the author was himself struck by the miasmata on which he operated. After his recovery, he resumed them at Vega de Lupia. But these experiments merely proved the presence of an organic matter deposited with the dew: it remained to ascertain the quantity of this matter. In admitting that the miasmata contained, like all other organic matter, hydrogen as an element, the weight of this hydrogen might be ascertained.

With this view M. BOUSSINGAULT passed a given weight of miasmatous air, well dried, through a glass tube heated red. At this temperature the miasmatous air entered into combustion, and its hydrogen formed water, which was received in a tube containing chloruret of calcium. By weighing the tube before and after the operation, the quantity of water (and of course hydrogen) was ascertained. But this is a very difficult experiment, as it was necessary that the air should be perfectly dry, to avoid any error.

In July 1830, the author made a certain number of experiments by this process: a volume of air varying from 305 to 310 grains frequently gave 0.150 of water, or 0.005 of hydrogen: as the heat continued to prevail, the ground became more dry every day: the quantity of water in the air diminished in proportion, and near the end of July not more than 0.0013 of hydrogen could be obtained. This result corresponded with what was known from experience relative to the insalubrity of the air in these climates. The mortality was always much greater immediately after the cessation of the rains.

In all these experiments the mal-sain air was received into the heated tube immediately after having passed through a long tube filled with fragments of chloruret of calcium. But if the air, before being exposed to heat, was wasted, or passed through sulphuric acid, it no longer gave any water: the organic matter remained in the liquid.

It now became an important point to ascertain if the air we breathe, even when freed from all miasmata, does not contain some hydrogen. On his return to France, the author investigated this subject. Formerly, hydrogen was regarded as a constituent part of the atmosphere, and meteors &c. were attributed to its combustion.

In some late researches on the carbonic acid of the air, M. SAUSSURE has been induced to suspect the presence of a combustible gas in the atmosphere; for in

detonating a mixture of pure hydrogen with atmospheric air deprived of its carbonic acid, he has always obtained some new carbonic acid. His experiments do not show the precise nature of the combustible gas which produced this acid, but it probably is either carbonated hydrogen or an oxide of carbon. M. SAUSSURE inclines to the latter opinion. From new experiments, M. BOUSSINGAULT adopts the former. These were made during the months of March, April, and May last, at Paris. The air which is discharged from the gasometer, traverses sulphuric acid, and is then dried in passing through a tube ten feet long filled with fragments of chloruret of calcium: from this it passes into a tube, *a*, filled with asbestos, imbibed in sulphuric acid to remove the moisture more completely, and thence into a similar tube, *b*, containing the same materials. The air is now passed into a red-hot tube containing copper filings, and here acquires a sufficient temperature to make any hydrogen which may exist enter into combustion. In the latter case water is formed, and collected in a tube, *c*; and from its presence, the presence of hydrogen in the air is concluded. The author proves that the water is derived from the combustion of hydrogen in the heated tube. It may be objected, that the water of the tube *c*, is derived from some moisture not removed by the calcium or sulphuric acid. It was to avoid this error that the author placed between this tube and the one connected with the furnace the tube *b*, which is disposed like the tube *c*, and has the same dimensions.

Now, if the tube *c* constantly augments in weight, while *b* does not, it appears evident that the water received in *c* was formed during the passage of the air through the heated tube.

Again, it may be said that the water in *c* was derived from the surface of the tube which is heated, or from the copper; but the tube and copper were heated before being fixed to the tube *b*, and several pounds of air were passed through before it was united to *c*.

Hence, air submitted to a red heat gives water, and the presence of a small quantity of hydrogen may be concluded.

The author details his experiments, but it will be sufficient to give his results. In the commencement of April, the quantity of hydrogen found in the air was a little more than 0.0001. After the middle of the month it was a little less. Perhaps this arose from the imperfection of the process.

We engage such of our chemists as may have time, to repeat the experiments of M. BOUSSINGAULT. They may lead to the most interesting results. If we remember right, Dr. PROUT observed that a portion of the atmosphere weighed more than it ought to have done, during the prevalence of the cholera in London. M. BOUSSINGAULT shows that the same volume of air may contain four times more miasmatous matter at one season than at another, and that this is indicated by the quantity of its water.

THE LANCET.

London, Saturday, Sept. 13, 1834.

THERE is no circumstance connected with the history of the COLLEGE of PHYSICIANS that appears so extraordinary as the liberality which has ever been displayed by that body in admitting candidates to examination for the *License*, as contrasted with the tyrannical restraints which have been imposed on the admissions to the *Fellowship*. On the first point the College has been entitled to general respect; with regard to the second, it has deserved and received universal execration. With the Charter which this institution possesses, it might have regulated the medical profession of England in such a manner as would have secured to the public a sufficient number of well-qualified practitioners, and it might at the same time have checked, effectually, and for ever, the villanous proceedings of professed quacks. But it has been the fashion for Presidents and Censors to sink into repose on the Collegiate couch,—to sleep over the laws which they might have wielded for the profession, or to interfere only for the purpose of disturbing their equals in rank, and their superiors in attainment. If the College had acted upon its charter, and had discharged its duty faithfully to the public and the profession, the miserable *Apothecaries Act*,

passed in 1815, would never have disgraced the statute book. But the College repudiated "Surgery" and "Pharmacy;" the vain meddlers set themselves up as the heads of the profession, and marked out different classes of practitioners,—lines of distinction,—which not only tended to confuse the public mind regarding claims of rank, but contributed to degrade all those members of the profession who had not the good fortune to be placed within the limits of the aristocratic boundary.

When Sir HENRY HALFORD was asked, "Did the College of Physicians oppose in "the first instance the Apothecaries Bill?" he answered, that "it was a matter of se-"rious deliberation; their counsel advised "them not to oppose it, and they acqui-"esced in that advice,"—the last advice we hope which the blockhead of an advocate was ever permitted to give. The Charter of the College, and the Acts by which that Charter was confirmed, conferred on the authorities of the College the power of inflicting penalties on all persons, in the British dominions, who practised Medicine without the license of the College,—except of course the privileged graduates of the two English Universities. But the Fellows of the College would not interfere, because, forsooth, they would not class themselves with "Apothecaries." They were ashamed to plead in a Court of Law that *Apothecaries* were Physicians. Hence every street contained its quack, and these circumstances stimulated the well-educated "General Practioners," in 1812, to petition Parliament for the institution of a fourth medical corporation. Out of the proceedings which thus originated, arose the Act of 1815,—an Act of which the first petitioners strongly disapproved; but which, as was truly acknowledged by Sir HENRY HALFORD, was "acquiesced in" by the College of Physicians. Never was a piece of grosser fraud practised on the legislature than in the passing of that measure. The Colleges of Physicians and Surgeons, and the Company of Apothecaries, were all playing, on that occasion, double parts—one in public, one in private; but they were influenced by a common motive,—avarice. The physicians, and their wise legal advisers, considered that the Act was so framed that it would secure to the physicians the whole of the medical practice in the metropolis and the country—that no other man could dare to prescribe in a "medical" case—that it would banish the surgeon from the entire field of fever, consumption, and all "internal" disease,—that it would chain the apothecary to his dispensing counter, and that it would rabbit his hand to the pestle and mortar. The Council of the College of Surgeons, crafty enough, but not a match for the Hags of the Hall, deemed the Apothecaries Bill a very advantageous instrument for *their* branch of the profession, conceiving it would furnish the groundwork for a similar measure regarding the practice of *Surgery*. Acting upon this opinion, in less than a year after the Apothecaries Act was passed, in went a "Surgeons Bill," sure enough, but it was at once scouted and rejected. This occurrence took place in the spring of 1816. The Old Ladies, therefore, completely outwitted their opponents and their "acquiescing" friends. They succeeded also in defeating the medical privileges of several of the Universities, and almost secured a monopoly of medical practice for their own Licentiates throughout England and Wales. If the College of Physicians had dared to discharge its duty, such a measure, we repeat, would never have been projected by any body of medical practitioners.

But the Fellows of the College were so puffed up and bloated with false notions of their own importance, that they could not be made to believe that the Drug Com-

pany of Blackfriars would inflict the slightest injury on the classical company that flourished in Warwick-lane! They were deceived, however, and when we still observe their obliquity of vision relative to the success of surgeons in general practice in this country, no one can be astonished at such a circumstance.

Let us look at the following question and answer;—

Ques. "Do you not think (to Sir H. HALFORD) that there is a great tendency at present on the part of the public to employ the general practitioner?

Ans. "There have always been one or more persons who have acquired credit with the public without being physicians."

This witness must really move about the metropolis with his eyes closed and his ears plugged.

Ques. "Do druggists practise physic?

Ans. "I do not know that; it does not come to my knowledge that druggists practise physic at all."

Here we have the PRESIDENT of the COLLEGE of PHYSICIANS who gravely states before a Committee of the House of Commons, in almost the same breath, that "*one or more* persons generally gain credit with the public without being *physicians*," and that he "does not know that *druggists* practise physic at all!" How many general practitioners there are who succeed in acquiring "credit with the public,"—not however without being "physicians," Sir HENRY, for depend on it that the surgeon who is constantly engaged in visiting the sick,—who attends to the preparation of his own medicines,—who is not so stupidly conceited as to be above seeing many of the minor duties of the sick chamber executed,—such a man, Sir HENRY, is your best physician,—is the man upon whose care the public may ever repose with the greatest degree of confidence. But the well-qualified "physician" of our aristocratic President, is he who passes many of the best years of his life at Oxford, where there have been no medical lectures and little medical practice. But the residence was *expensive*, and the cost made the title of M. D. respectable.

Ques. "What advantages, in your opinion, result from confining admissions into the College almost exclusively to the graduates of Oxford and Cambridge?

Ans. "I know of no advantage beyond the opinion that the world may have of those persons, from their education, being more likely to be accomplished physicians; I know no reasons upon earth but that; the Fellows have no privileges whatever that a Licentiate has not.

Ques. "What advantages to the public do you think result from that exclusion?

Ans. "Men are tempted to seek the best education that the country can afford.

Ques. "You mean the best literary and scientific education; or do you mean the best medical education?

Ans. "The best preliminary education to the study of physic.

Ques. "Do you think the best preliminary education and the best medical education is given to the graduates of Oxford and Cambridge?

Ans. "I believe that the physic that they may acquire at Oxford and Cambridge is undervalued by those who reproach the Universities for not being schools of physic; but that is of very little importance if they have their preliminary education. They will go and find physic wherever it is to be found, afterwards.

Ques. "Then it is to the superior preliminary education they acquire at Oxford and Cambridge that you attribute any advantage which arises from the restriction alluded to?

Ans. "I believe so; they have no other claim to preference but that; they have no advantage over the Licentiates whatever; the license to practise is necessary to the Fellow as well as to the Licentiate; they are both Licentiates in the first instance; they are elected to the fellowship afterwards, and with some degree of preference because they are known to have passed through an ordeal, to have been domiciliated in the College, subject and subjected to all the rules of that College, as well as the laws of the University. As to Licentiates who come to us, we have no knowledge how they have spent their time before they began the study of physic, or where they studied, for they may have got a degree at a university without having resided there.

Ques. "Is it to the literary or the scientific part of their education, or to the moral discipline, that you refer?

Ans. "Both one and the other; both the moral and intellectual discipline they undergo at the English Universities.

Ques. "Do you mean that those are the only seats of learning in which due preparatory education is given to those who intend to enter into the medical profession?

Ans. "By no means the only place; I know those places well, and I know there is an opportunity of undergoing moral discipline, which the great body who come for a license have no proofs to give that they have undergone.

Ques. "Have proofs ever been asked for; have the College ever endeavoured to inform themselves as to the scientific or literary education, and as to the moral habits and discipline of other parties who have not been educated at the two English Universities?

Ans. "You may learn their scientific and literary acquirements by examination; but not their moral qualities. I am glad that question is asked: it is upon that ground that we insist upon the Licentiate remaining a certain number of years a Licentiate before he can be admitted into the fellowship; for the College requires a residence at a university two years; it requires that a person shall have taken a degree after having been resident at a university for two years; then the College expects that they shall remain in the list of Licentiates ten years: that makes twelve. We, who go to Oxford or Cambridge, are eleven years before we get our degrees, so that that brings it pretty nearly to the same standing. In one case, the head of the College at which the person has resided bears testimony to the moral character of the individual before he presents him for each of his degrees; we think this is counterbalanced by insisting on a Licentiate remaining, in the eyes of his brethren, to have his conduct observed, before he is eligible into the fellowship.

Ques. "You will have no objection to giving the same facilities to admitting Licentiates, not graduates of the English Universities, into the College of Physicians, provided you could inform yourselves of their moral habits and of their scientific and literary education?

Ans. "It is almost impossible to get this information.

Ques. "You prefer the restriction of admission into the College to the graduates of Oxford and Cambridge, because you can better inform yourselves of their literary and scientific education, and of their moral habits?

Ans. "Yes.

Ques. "Provided you could inform yourselves of persons, not graduates of the English Universities, being equally well qualified by good moral conduct, and by literary and scientific acquirements, ought you not, consistently with your own arguments, to give to them the same facilities of admission into the College which you give to the graduates of Oxford and Cambridge?

Ans. "If I could have the same satisfactory proofs, I would admit them in the same manner.

Ques. "It appears from the Return made by the College to Parliament, during the last session, that from 1771 down to the present time, there have been 147 graduates of Oxford and Cambridge admitted; that in the same period there have been 19 Licentiates admitted on the recommendation of the President, and four Licentiates recommended by the President rejected: thus, in 63 years, it appears that there have been only 19 Licentiates admitted, and that of those, seven have been admitted in the last 10 years. Do you think that amongst the Licentiates who have practised in London during that time, a much larger proportion than 19 have not been qualified, both by their education and moral habits, to be admitted into the College?

Ans. "Probably.

Ques. "If there are many more qualified by their education and moral habits to be admitted into the College, how do you account for a greater number not having been admitted?

Ans. "I account for it by the acrimony of the disputes between the Fellows and the Licentiates, and the violence with which the latter pursued their claims to the Fellowship; the repeated decisions of the Court of King's Bench had determined the question of right.

Ques. "Can you account for them not having been admitted?

Ans. "No otherwise. We have been more liberal lately in their admissions than formerly.

Ques. "Do you not believe the fact to be, that there always has been a very large proportion of the Licentiates practising in London, who, from their moral character, from their scientific and literary acquirements, from their medical knowledge, and their extent of practice, have been eminently qualified to do honour to the College, if admitted thereto?

Ans. "No doubt about it; but no injury has been done to them: they have been able to make their fortunes just as well as the Fellows have, and they have always done it. I see no advantage to the Fellow, except in the opinion the public may have in favour of a person who has been well educated at one of our own uni-

versities, instead of not knowing where he has been educated.

Ques. "Does not the practice of a physician depend upon public opinion?

Ans. "Certainly, in a degree."

Precisely the evidence we should expect from the lips of Sir HENRY HALFORD. The first questions having been proposed without any introduction, and the querist having pounced on Sir HENRY at once, he succeeded in eliciting a reply which might have been somewhat differently framed if the examinee had been allowed a little time for thought or preparation. The question and answer are well worthy of a second reading.

Ques. "What advantages in your opinion result from confining admissions into the College almost exclusively to the graduates of Oxford and Cambridge?

Ans. "I know of no advantage beyond the opinion that the world may have of those persons, from their education, being more likely to be accomplished physicians. The Fellows have no privilege whatever that the Licentiate has not."

Sir HENRY was here fairly caught in the trap set for him, and the truth was squeezed out, *nolens volens*. It will be observed that the word "advantages" in the question had a general, not a specific, reference; but Sir HENRY, taken by surprise, was incapable of stating that any benefit resulted from selecting the candidates for the fellowship from the graduates of Oxford and Cambridge, save that of inducing the public to entertain an opinion that the Fellows of the College were therefore the "more likely to be accomplished Physicians." He *then* knew no reason on earth but *that*, though he could afterwards state that they gave a preference to the graduates of English Universities, because the domiciliating at one of their Colleges was a "security for the *good moral habits*" of the candidates! Such having been the reply to the former interrogatory respecting the favoured graduates of Oxford and Cambridge, the querist asked,

"What advantages to the *public* result from that exclusion?"

This is a brief and remarkably plain question. What was the answer to it?

"Men are tempted to seek the best education that the country can afford."

Afterwards, this was explained to be a preliminary education — "preliminary," said Sir HENRY, "to the study of physic." "Whether they get physic or not at "Oxford," said he, "is of very little "consequence. They will go and find "physic afterwards wherever it is to be "found." It is evident therefore that it has been the whole object of the College to make the admissions into the Fellowship as expensive as possible, in order that the number might be limited, and that the public might therefore be induced to form an opinion that only a few men had sufficient ability to become such amazingly erudite and accomplished physicians. Thus, throughout, a double fraud was practised on the profession and the public, and this very College, which was founded to uphold the dignity of the profession, has been engaged in a system of trickery and quackery of which it might justly be ashamed. The object has been to keep down the Licentiates, and to represent them as individuals who are destitute of that knowledge which would warrant the College in admitting them into the Fellowship. Sir HENRY, however, is at a loss to understand what advantages the Fellows enjoy over the Licentiates, except the trifling one of sharing largely in the favourable opinion of the world. With most admirable tact he was asked on this point:—

"Does not the practice of a physician depend upon public opinion?

Ans. "Certainly,—in a degree."

"*In a degree*?" Why not have said in a glass bottle, Sir HENRY? The question was a disagreeable one; the word "*certainly*," was a correct and honest answer, but the words "*in a degree*," were added

in the hope of effacing a portion of the impression which the first three syllables of the sentence would inevitably leave on the mind. Public opinion is a medical practitioner's only reliance. If he have no character with that portion of the public with which he happens to be connected, he can have no chance of success. The College knowing this, as we are told by Sir HENRY, have invariably endeavoured to obtain for the *Fellows* the reputation of their being the more "accomplished physicians,"—of being the best-educated men in the country; and yet, when he is reminded of keeping the Licentiates out of that body, he replies, that "the College does not put them in a lower scale." No; not even by resorting to that system of routine exclusion which, he confesses, will turn the balance of the world's prejudice in favour of the Fellows!

The evidence, however, which we are about to give, bids defiance to subterfuge, and demonstrates the objects and intentions of the Fellows, with respect to the Licentiates, in characters which cannot be misunderstood. Having been questioned on the subject of Midwifery, he was asked the following questions:—

Ques. "Do you not think that since, according to your own statement, *Midwifery* forms one of the subjects of examination at the College, it would be desirable to repeal that by-law which excludes any person from admission into the College who is engaged in the practice of Midwifery?

Ans. "No; I do not think it is expedient,

Ques. "Will you state your reasons?

Ans. "I think it is considered rather as a *manual* operation, and we ought to be very sorry to throw any thing like a discredit upon the men who had been educated at the Universities, who had taken time to improve their minds in literary and scientific acquirements, by mixing them up with this manual labour. I think it would very much disparage the highest grade of the profession to let them engage in that particular branch which is a manual operation.

Ques. "If a gentleman engaged in that branch of the profession should happen to be a man of good moral character and good education, do you think it would tend to lower the College of Physicians by admitting him to the fellowship?

Ans. "I do not say the admission of a single individual would, but if it were generally done I think it would."

Very well. He thinks that the Fellows would be degraded by admitting practitioners of Midwifery to the Fellowship though men of good moral character and good education. He thinks it would "disparage the *highest grade* of the profession." But observe what he stated soon afterwards!

Ques. "Have the laws of the College ever been suspended in favour of any individuals practising *Midwifery* to admit them as Licentiates of the College?

Ans. "No; we do not object to *Licentiates* practising; we are *glad* if a Licentiate will practise Midwifery."

Was there ever witnessed a more disgraceful exposure? The Fellows are not permitted to practise Midwifery, because forsooth, it would disparage them in the eyes of the public, as it is a "manual operation," but the President and Fellows are "glad,"—they rejoice,—if the *Licentiates* will take upon themselves an office which is calculated, in the opinion of the Fellows, to lower them, the Licentiates, in the estimation of the community. A confession of this kind, from the lips of Sir HENRY HALFORD himself, sustains all the allegations which have been made by the Licentiates against the unjust treatment they have experienced from the ruling authorities of the College. We say nothing of the folly, the absurdity, the narrow-mindedness of such a man as Sir HENRY believing that a practice which was pursued by a HUNTER and a DENMAN of past times, and by a BLUNDELL and a DAVIS of the present day, can be of such a nature as must tend to diminish the public respect for the talents and utility of the "highest grade of the profession." Sir HENRY HALFORD himself—this President of the College of Physicians, is absolutely unworthy to be named in the same page

with such men, and yet he has dared to say that he is "glad" when a Licentiate undertakes to discharge a professional duty, which, if executed by a Fellow of the College, would degrade his scientific character and sully his respectability!

At this point we shall adjourn the enquiry, at the same time entreating the Licentiates of the College of Physicians, and all the English practitioners of Midwifery, to examine, with that attention which the subject merits, those extracts from our report of the evidence which we have inserted in this article. Let them never forget that although Sir Henry contends that the business of the accoucheur, —of the moral and well-educated accoucheur,—would degrade a *Fellow* of the College, yet that the President and his associates in intrigue rejoice and are "glad" when a *Licentiate* engages in the practice of Midwifery.

The Dublin Practice of Midwifery. By Henry Maunsell, M.D., Superintending Accoucheur to the Wellesley Female Institution, and Lecturer on Midwifery at the Park-street School, Dublin. London: Longman, 1834, 12mo, pp. 244.

The following story has often amused the readers of that ingenious author, Mr. Miller: A colonel of militia, who had suddenly been promoted from the park to the field, was reviewing his regiment for the first time, by the book of drill, not being "up" enough in the evolutions of human troops to do so from memory. Arriving at the bottom of page 6, he commanded the men to "Shoulder arms!" The order was obeyed. The colonel promptly turned over leaf, and cried aloud, "Unfix bayonets!" A pause ensued, and the exclamation was repeated. The pause, however, continued, when the orderly sergeant advanced from the ranks, and making the salute, softly informed the colonel that the bayonets could not be unfixed while the arms were shouldered. "Ah!" said the colonel, "I have turned over two leaves at once," and turning back to page 7, ordered the arms into a position more consistent with the proper train of manœuvres, and proceeded afresh. But the *pause* had an awkward effect.

The story conveys a good moral, which we will endeavour to convert to the use of all lecturers who may preface their writings with such a sentence as forms the introductory paragraph of Dr. Maunsell's epitome; *videlicet*:—"Those who "are conversant with the teaching of obstetric medicine, must have felt the "difficulty of answering a question commonly put to them by students:—*What "book do you recommend me to take to the "lying-in room?*"

And does the Doctor really recommend any book to be taken to such a place? Does he really recommend any student to play the midwife, book in hand? His reply is furnished in the production of the "Dublin Practice of Midwifery," and the design of supplying a chamber guide for unproficient midwives, has evidently prevailed with him from page 1 at least to page 209, where he observes that a certain classification of puerperal fever is "rather suited to the dissecting-room than the sick chamber," and therefore transfers it to the present work. Is the Doctor afraid of no awkward pauses as the student turns from the parturient woman to the "Dublin Practice?" Does he fear the production of no alarm in the mind of the patient who observes such a literary consultation? The impropriety of exciting her fears crossed the Doctor's mind when he came to speak of the preliminary duties of an accoucheur. "The only matters," he observes, "necessary to be taken to a "patient's house are, a catheter, a lancet, "and a little tincture of opium. All other "obstetric apparatus (*other* obstetric ap-"*paratus!*) I would strongly advise to be "left in the practitioner's study. If accidentally seen in the lying-in chamber, "they cannot fail to excite alarm."

But the moral of Mr. Miller's story must not escape us. We intend to give an illustration of the possible effect of using an obstetric drill-book, which will be more useful than that which Joseph has given of reviewing by a regimental one.

The student whose pocket is better filled with knowledge than his head, is packed off, say, to do the best he can for a parturient mother and her anticipated infant. The labour is stayed, perhaps, by

inertness of the uterus. In such case what shall the young gentleman do? The oral advice of Dr. Maunsell has been forgotten; but being fortified with the doctor's "Dublin Practice," he forthwith consults it, and turns to page 117, where, under the head of "Original Inertness of the Uterus," he finds the following statement:—"Aperients and enemata are particularly useful "in this variety, often (especially the latter) exciting pains. Opium has been recommended in very large doses; but when "given thus, I have"—student here turns over leaf—"stertorous breathing, without at all affecting the uterus—perhaps even paralysing it."—"Alas!" says the student, emptying his pockets, "I have neither aperients nor enemata here; nothing but the catheter, the lancet, and the tincture of opium, advised at page 89. I should like to try the ergot, of which so much has been said, but have got none. Opiates I recollect really have often been *recommended*. The woman shall have the tincture. Even if she have stertorous breathing I can't do much harm, as the book says that large doses may be given without at all affecting the uterus."—Happily but little" of the tincture is at hand, and he ultimately departs without delivery; and on explaining his practice at home, discovers that, like the colonel of militia, he had "turned over two leaves at once," and run page 117 into page 120.

While this labour was thus proceeding according to one copy of the "Dublin Practice," another case awaits an accoucheur, and a second gentleman is dispatched from the doctor's house, with another copy of the work, Dr. MAUNSELL having been previously asked by the young student "What book he could re"commend him to take to the lying-in"room." Arrived at the house, nature proceeds as well as can be expected up to the separation of the after-birth, when hemorrhage ensues, and the ignorant but yet not wholly unprepared accoucheur, turns as pale as his patient. "*The book!*" says he, suddenly, and forthwith whipping it from his pocket, turns to page 175, at the bottom of which he reads,—"Hemorrhage occurring after expulsion of "the placenta. In some instances this is "occasioned by"—here he turns over leaf—"*spasmodic contraction.*" "The devil it is," says he; "what *can* I be at? Ah! the uterus must of course be relaxed. "Belladonna's the mark. Home with us, "and turn to the Pharmacopœia." Too late to remedy the mischief, he hears from some old orderly sergeant of medicine that relaxation of a uterus which has not contracted, is a vain attempt. Finally, he learns that Dr. MAUNSELL really declares in his book, that such hemorrhage "is occasioned by *the after-birth being removed too hastily*, BEFORE the *uterus is inclined to contract.* The student finds, in fact, that he had accidentally "turned over two leaves at once."

A third student, placed upon similar trial, involves himself, though not the unfortunate patient, perhaps, in a similar error. He gets hold of a "Maternity" patient, and thinks it a good occasion for trying the forceps, which, in defiance of page 89, he has ably brought with the catheter, lancet, and laudanum. Puzzled during some opportunity for their use, he turns to the chapter on obstetric operations, and at the bottom of page 131 learns that "often, the mere introduction will "excite a pain, and he may then move "the instrument slowly and steadily"—when, turning over, he finds the leaf commencing, "By some authors, the pro"priety of using the instrument as a lever"—("a *lever?*" quoth he, "what the "plague! is it ever used as a lever?") has "been impugned, and its name has been "attempted to be changed into *extractor*,"—very unjustly, he thinks, and would be glad if the forceps *would* become an "extractor."

We pray Dr. MAUNSELL to smile with us. Any book designed for the same end would deserve the same exposition. We may not always have said so much, for the student may be wiser with than without a bed-chamber guide. But a new era breaks on us, and if we ever have had patience with such books, the moment is gone. Besides, what a volume for such a destination! Maxims only, and those squeezed into the pettiest compass, can be permitted to ride with the catheter and lancet. Think of prefacing a chamber compendium with seventy-seven duodecimo pages of anatomy and physiology, before one word is said of the "separation from the gravid uterus of the *foreign* substances, so to speak (so to speak, indeed!) contained within it."!

Putting aside the objections we have urged against the compilation of works for the exigencies of a lying-in or sick-room—because the inmates of such rooms ought not to be placed beneath the medical care of exigent hygeists,—we may say that the volume of Dr. MAUNSELL is a useful epitome of the obstetric art,—simple in arrangement, and plain in style, and that it will serve the purpose of those who are anxious to obtain an outline of the duties of an accoucheur—not, certainly, for consultation in the lying-in-room. We are entitled to ridicule that object without compassion.

A Practical Treatise on Lepra Vulgaris, with Cases that have uniformly yielded to the Plan of Treatment laid down. With Observations on Psoriasis. By EDWARD BECK, M. D. Ipswich, 1834; 8vo. pp. 74.

DR. BECK treats lepra on a plan which, he says, has been uniformly successful in *his* hands, and which he wishes, therefore, to be submitted to extensive trial. Arsenic by the time it has made an impression on the disease is ready to create a new one,—perhaps without curing the first, and the *solanum dulcamara* has not answered the expectations excited in its favour by BATEMAN. All remedies but one have been followed by repeated failures. The desideratum is promised by Dr. BECK, and thus supplied in the foregoing volume, which, as emblems and mottoes are selected from every source, might, not inappropriately, as healing is a sacred art, have had its title-page graced by a Noah's ark, and these words,—"Thou shalt pitch it *within* and *without* with pitch."

When the lepra is of long standing and severe, a preparatory antiphlogistic treatment is required, with a view to reduce the constitutional irritation of the patient. This done, the specific plan consists in applying the following liniment to the parts affected:—℞ *Picis Liquid. Sulphuris; Adipis. Præpar.*; sing. unciam. Misce. The following pills are to be taken regularly, at the same time:—℞ *Picis Liquid*, unciam dimidiam. *Flor. Tritici*, q. s. Misce, et divide in pil. sing. gr. v. 3 ad 6, ter in die sumantur. Where the irritation of the lepra is great externally, mix but half an ounce each of the pitch and sulphur, with an ounce of the oleaginous material. Moreover, if the irritation attending the application of the milder preparation still be considerable, it should remain on the eruption but a few minutes, and be then wiped off with soft linen. Increase of pain is felt for a short time, but that soon subsides. In three or four days the stronger liniment may be used, and then may remain on all night, or even continue on for days. The less frequently it is washed off the better, for much local irritation will thereby be saved. Every third day will suffice to apply it afresh. As people with a delicate scent may dislike the smell during the day, the liniment may be applied at night and washed off in the morning. Where the lepra is in patches, the ointment may be laid on thickly, beneath two or three pieces of lint, having adhesive plaster hermetically superimposed. This will confine the odour, and may remain undisturbed for days. Small spots will yield to a smearing of the ointment on them, and this mode may be followed when a whole limb is involved in the disease. Envelopes of linen are proper where entire extremities are anointed.

The pills may create headach and nausea. If they do, two at first, given thrice daily, increased to six *gradatim*—more than which will affront almost any stomach—may be prescribed. The bowels will be relaxed by them, but the stomach ought to be prepared for the medicine by mild aperients, and both pills and ointment should be continued for a considerable time (three, four, five, nay more weeks) after the disease has disappeared. In three weeks the eruption has sometimes almost entirely departed, notwithstanding which an arrest of the treatment has allowed the lepra to revive with increased severity.

As general admonitions, Dr. BECK advises that the ointment should be applied early in the disease. The pills will *begin*, but the ointment must *perfect*, the cure, just as mercury is demanded to chancres, even when the system is saturated with the metal. The nitrate of silver, externally, will remove a leprous patch, but the pills are essential to cure the disease, whether the nitrate or the pitch be used on the surface. As the taste of the pills is revolting to patients, they should be taken "at the point of a tablespoonful of gruel," by which the asinthiacal flavour will be masked. In all cases adapt the treatment to the state and stage of the disease, which will yield much sooner on the upper than on the lower part of the body.

Cases are appended by Dr. Beck, which chiefly show, in addition, the means adopted in aid of the specific treatment; and the author has supplied some interesting points illustrative of the history and progress of the disease. In one of them, successfully treated (like the rest), the patient's entire person was covered with the leprous eruption, "a very unusual circumstance." Such a case will be found in No. 457 of this journal, the eruption in which was stubborn to every thing but the ung. picis liquid, which was the last of many remedies applied, but the external use of which does not appear to have perfected a cure. Some remarks follow on *psoriasis*, in which the author recommends as a liniment a combination of *camphor*, with the *cerat. cetac.*, saying, "I have found "no application so serviceable as this, and "no internal remedy equally efficacious "with sulphur and soda." In a severe case of *psoriasis diffusa*, after an antiphlogistic treatment, and a branny ablution of

the parts, had been employed, the following were prescribed, and a perfect cure effected:—℞ *Sulph. Præcip.* drachm. divid.; *Sodæ Subcarb. exsic.* gr. v. Misce. Ft. pulv. his vel ter in die sumend. ℞ *Camph. pulv.* (subsequently increased); *cerat. cetac.* unc. Misce. Ft. linim. part affect., nocte et mane applic.

The volume forms such a contribution to medical science as a conscientious practitioner should make; but since books are now-a-days so difficult of dissemination, we wonder the author did not present his paper to one of the medical journals; ten of our own pages would embrace the seventy-four of the Doctor. Practitioners who wish for more minute particulars of the Doctor's plan of treatment must consult the volume itself. "Want of attention to minutiæ," says the author, very properly, "will often cause means "which have been perfectly successful "in the hands of one individual, to fail "entirely in the hands of another."

SECOND LETTER

TO THE

MEMBERS OF THE DUBLIN COLLEGE OF SURGEONS,

On their Present Condition and Collegiate Duties as Members of that Institution.

GENTLEMEN,—I addressed a few lines of exhortation to you, to arouse you, in some degree, from indolence and apathy, to some energy of purpose, as well as a proper sense of the high and important duties that devolve on you as members of the College of Surgeons.

I hope that that communication has not been without effect, as, however galling the statements therein contained may be to the corruptionists (whose characters already begin to be appreciated even by the community at large), I well know that those statements rather fall far short of than transcend the genuine feelings of the majority of our profession. Another moment of leisure offers, and I resume my pen in the humble hope of doing "the good cause" some service, that cause which, you may rest assured, not only deeply involves the very existence of the College, but also your nearest and dearest rights, both as members of it, and of a profession by which you hope to gain a respectable existence and honourable distinction.

Unreflecting respect for, and passive submission to, the opinions and measures of others, although principles sometimes praiseworthy, and the accompaniments even of well-regulated minds, are too often but the effects of imbecility, or the apology of apathy and indolence. To excuse or justify such a state of mind, in men embarked in a public cause, or engaged in public duty, there should be exhibited not only sterling talent and sound sense, but also *disinterested* probity in a long series of honest, useful, and *general* measures, emanating from, and perfected by, those in whose favour it is exercised. Are there any traces of such qualifications in that presumptuous, self-appointed, and self-styled ministry, who, by still holding the reins of management, not only impose on your generous but mistaken forbearance (which they will continue to do, if suffered, to the very end), but insult common sense, and deeply injure your most vital concerns? No! in the whole course of their lengthened and reckless legislation, there is no vestige of one or other. All their measures, destitute alike of genius, talent, liberality, probity, are calculated to forward only their own narrow views, their own selfish interests. Does then your inaction depend for cause on native imbecility of intellect, or dulness and apathy of spirit? Are you so stupid as not to be able to comprehend your own interests; so indolent as to be incapable of a single exertion to forward them; so devoid of generous sentiments as to make no stir in the cause of public virtue, science, and improvement? Will you rather risk everything in gratification of good-natured indulgence and pathetic forbearance, matters that render you objects of contempt and derision to those who impose upon you?

But, these are moments of paramount importance, moments that may never be recalled, that imperiously demand the strongest exercise of our most mature judgments, upon which hang the fate of the most important interests. In these it is little short of treason to surrender the trust committed to our hands, to stifle our own judgments, to abandon our own opinions, in favour of any set of individuals, much less of those who, by their measures, have never been able to conciliate our approbation, but have been convicted of many a treacherous betrayal of our confidence.

Such a moment has now arrived to the College. Never in its whole history was there a juncture of more vital importance than the present. Upon the prudence of present measures, and the dexterity and quickness with which you seize on present opportunities, hang not only the independence, honour, and utility of the College, but even its very existence, your rights and privileges depending thereon. Unless you grapple with affairs at once, unless you rouse yourselves from your strange and unaccountable inaction, unless you exercise that power, which though within your reach at a moment's notice, yet lies in marvellous and neglected abeyance, unless you open your eyes to surrounding treachery and look your betrayers in the face, unless you *now*, by one vigorous and determined effort, assert the independence of the College—all is lost. Exercise your rights and privileges *at once*, or abandon them for ever. Beware how you are gulled by the plausible sophistry and apparent fair dealing of a deep and designing C—k, caught by the spurious liberality of a flippant and superficial H—n, deceived by the constant undercurrent of a tortuous, subtle, and oily C—s, blinded by the bombastic brilliancy of a renegade K—g, driven with disgust from action by the vulgar venom, the malignant billingsgate of a mountebank J—b, or the combined influence and jackal conduct of their respective unmanly, unprincipled, ungenerous retainers. Beware how you place confidence any more, not in suspected friends, but in convicted enemies; they are the same one and all. This moment they are in treacherous treaty, or proposed with their perfidious league. Members of the College, you are

betrayed and sold. But one thing can rescue you from destruction,—your own energy and activity; and that only if put into immediate operation. This is the only means that can antagonize their measures; and this they well know, when they cry out, as they did at the late adjourned quarterly meeting, "Wait awhile;—is this a time to make any change in our affairs? No, rather expect the denouement of the Committee of the House of Commons, when the evidence shall be printed,—when we shall know the entire cause,—when the ultimate intention of the legislature shall be declared; then we can see our course clearly, and shape it accordingly." Aye, wait until it be too late to stir; wait until their overtures are received; wait till their measures are perfected and irrevocable; wait until they, declaring themselves your organs, the representative of your sentiments and your opinions, in fact, as they have dubbed themselves "the College," have irretrievably established their own power, but your discomfiture and ruin.

Power and influence are sweet in themselves, but fortified as they are in their case with interest and gain, they will make every exertion *per fas et nefas* to retain. They will easily effect their purpose, should they succeed in blinding you and keeping you in unreflecting ignorance and inaction. Stupid and ignorant though they be, yet are they cunning enough to perceive that increased knowledge and the spirit of the times are at length beginning to take effect in our body, though, strange to say, their influence has been of slower growth here than almost anywhere else. From this new element in our meetings, and upon which they quickly fasten a false appellation, they have received some startling shocks.

Their antagonizing policy is and has been simply this: on the one hand to fence themselves strongly in present corruption, and to resist, with their utmost struggle, every improvement and innovation, no matter how valuable in itself or strongly recommended; and, on the other, they malign and defame our meetings. These they have represented as noisy and disorderly debating shops, where time is wasted in vain and useless discussion, where deliberation is made impossible by number and tumult; where the spirit of faction reigns in anarchy and confusion. To permit the transaction of business, they call for a diminution of number; the appointment of visitors, or council, or extraneous board, upon which they stipulate, that they shall be appointed: they declare that appointment would be popular to all but a bigoted faction, whose power is in the inverse proportion of their noise and factious disposition; that they are the College, and should be treated as the College, and have power, influence, and *authority* to be treated as such. Thus, if unresisted, they establish what they have long desired, they change your council-chamber into a register-office, to record and give legality to their acts and measures; they fix themselves perpetually and firmly on what is now their precarious eminence, held at your beck, and they load you in your very teeth with ruin, infamy, and scorn.

But these measures during their progress must of course be most carefully guarded from your knowledge, for otherwise even *you* would be startled, and would be "up and doing." Hence their disinclination, at the meeting above alluded to, to enter upon the particulars of their evidence and conduct in the committee-room. Hence the discrepancy of their conduct in this respect from that on a former occasion; these hours were given up to the report, which was most justly called for and most freely given; because then they could say what they pleased and what answered their purpose; but now things are quite changed, not a word must be whispered, we must "wait a while," from one quarterly meeting to another, and so on; because other witnesses have been present, and the whole truth should come out, because the deceitful mask would be torn from their uncandid brow, and themselves exhibited in all the naked deformity of dishonest and selfish intrigue.

With this view too of increasing the unsuspecting film that almost blinds your mental vision, you heard at that meeting the would-be-liberal declaration of two of your most wary politicians. For my part, I should have no objection to remove the ballot altogether, and yet these consistent liberals mustered all their forces, retainers, allies, and mercenaries, all to resist with might and main the restoration of the by-law relative to the election of members to its former condition, and they gained a victory, such a victory! by availing themselves of a factious debating trick of adjournment. They had a majority of four—a majority of four when the very citadel and strong-hold of their corruption was stormed. This is not very disheartening. Another such victory and they are ruined; but they had been beaten all out by a larger majority, if the liberals had mustered with equal spirit.

Meantime but one interest is carefully looked after, and the preservation of which is a sacred "sine qua non," the school, the College school, that which they insultingly call your school. What interest have you in that marvellous institution? Are you a corporation of schoolmasters? With this query I conclude at present, and am yours, &c.

A MEMBER OF THE DUBLIN COLLEGE.

Dublin, Sept. 2nd, 1834.

FIRST RESIGNATION AMONG THE TRAITORS AT THE ALDERSGATE-STREET DISPENSARY.

To the Editor of THE LANCET.

SIR,—The man whose early history you gave an account of in your Journal last year, and whose peculiar fitness to succeed such men as Drs. BIRKBECK and CLUTTERBUCK you pointed out, has just resigned his situation in the *Aldersgate Street Dispensary*.

Dr. Whitsed was the first to oppose himself to the general feeling of his medical brethren, by consenting to take office, and is the first to turn his back upon the institution. Keep your eye upon his successor, if one be found, which I very much doubt. Dr. Yates and his colleagues do not, I am told, feel very comfortable in their situations. Shunned by their professional brethren, and pricked by their own consciences, they are beginning to doubt the propriety of the course which they pursued.

Mr. Herring, notwithstanding the queries addressed to him by the Parliamentary Committee, still holds the joint offices of treasurer and druggist to the dispensary! Your

CONSTANT READER.

London, Sept. 8th, 1834.

To the Editor of THE LANCET.

SIR,—Dr. Whitsed has resigned his office at the Aldersgate-street Dispensary. When he swallowed the bitter insult offered to the profession, it was sweetened by a stipulation, that in the event of his be-

coming a candidate, he should retire whenever he chose, with the liberty of writing after his immortal name "Consulting Physician to the Aldersgate Dispensary." The treasurer consented. When the committee met on Thursday last, Dr. Whitsed tendered his resignation, which was graciously accepted. This, however, was not done until the brother of the treasurer had provided another candidate, and made the election of that candidate secure by a canvass. The successor to the immortal Whitsed is to be a Dr. Hall. I am, Sir, your obedient servant,

C.

Aldersgate-street, Sept. 9th, 1834.

ST. GEORGE'S HOSPITAL.

AFFECTIONS OF THE SPINE.—In these cases (said Mr. Brodie), the only use of issues is to prevent the formation of abscesses. They are not, and cannot possibly, be of any use after abscesses have once formed. When that is the case, the best course is to let the patient lie in perfect repose in the horizontal posture, and wear a pair of stays (made for the purpose) that will support the spine well. Patients in these cases likewise have frequently many anomalous pains about the lower region of the bowels, the lower extremities, &c., which I can only suppose to proceed from nervous influence carried along the course of the lumbar nerves, chorda equina, &c. The pain which some patients in these cases suffer from so severely, generally arises from the lumbar abscess being in the neighbourhood of the spinal chord, or nerves on the outside of the vertebræ. I remember a case in which the patient suffered very severe pain in one of the lower extremities, and on examining the body after death, it was observed that the abscess was connected with, and pressed upon, one of the lumbar nerves.

CORRESPONDENTS.

Mr. Gleg.—The recipe is no secret. With a very unimportant difference in one ingredient, it has been publicly compounded for years.

Edinensis.—The advice will pass exactly for what it is worth, with a full knowledge, doubtless, on the part of every reader, that the mode of treatment detailed by our "Edinensis" is an alternative open to those who do not think that the other plan is demanded by the symptoms.

We cannot undertake to modify the expressions employed in the letter of *Mr. H. O.*, which in its present form would, if published, however correct in facts, render us liable to a prosecution for libel. We, therefore, refrain from giving it insertion. Besides, if the parties are not acting in violation of any law, we have little right, as journalists, to interfere, the contract being one of a strictly private nature. If the druggist thinks it advantageous to his own interests to refer publicly (as Mr. O. says he does) to the "contract," let him send a copy of it to THE LANCET Office, and we will decide whether it would be beneficial to himself or the profession to publish or make allusion to it in our pages.

Dr. B. of S.G. has been misinformed. No such article has either been prepared or contemplated, whatever opinion circumstances may, on another occasion, induce the editor to express on the subject. Would Dr. B. favour the editor, since a personal application has been made, with the name of the party who has made the statement alluded to, that statement being in every respect false.

Experientia. Proof of having been in practice previous to the passing of the Act of 1815 is required, and no existing law would be violated by acting as a physician in any case.

We intended to publish entire the first communication of *Mr. Godrich*, but have been able to afford space for his second letter only for the present.—We shall insert the letters of *Mr. Todd* and *Mr. Knowles.*

We have received the Report of the Special Committee of Governors appointed to inquire into the efficiency of the medical department of *Christ's Hospital*, with Mr. Plumbe's pamphlet, and several manuscript communications on the same subject, but are unable to notice them farther this week.

We shall publish all but the personal parts of *Mr. Ben's* controversial letter.—An accident has hitherto prevented the appearance of *Mr. Battyn's* communication.—The communication, &c. of *Mr. Brougham* has reached us.—*Mr. Dermott's* remarks on a new mode of applying moxa in tic douloureux has been received.

NOTICE.—The lecturers and teachers at the public medical and other schools of the metropolis, who may send the ensuing October prospectuses of their several establishments and courses to our office in the early part of the ensuing week, will have them included in the list of London medical schools, which will be published in the *forthcoming Student's Number of* THE LANCET.

ERRATUM.—The two paragraphs relative to the administration of salt-water emetics by Mr. BIRD, which were subjoined to the "Clinical Observations" of Mr. WARDROP in last week's LANCET, page 831, were accidentally transposed in print. The first should be last and the last first.